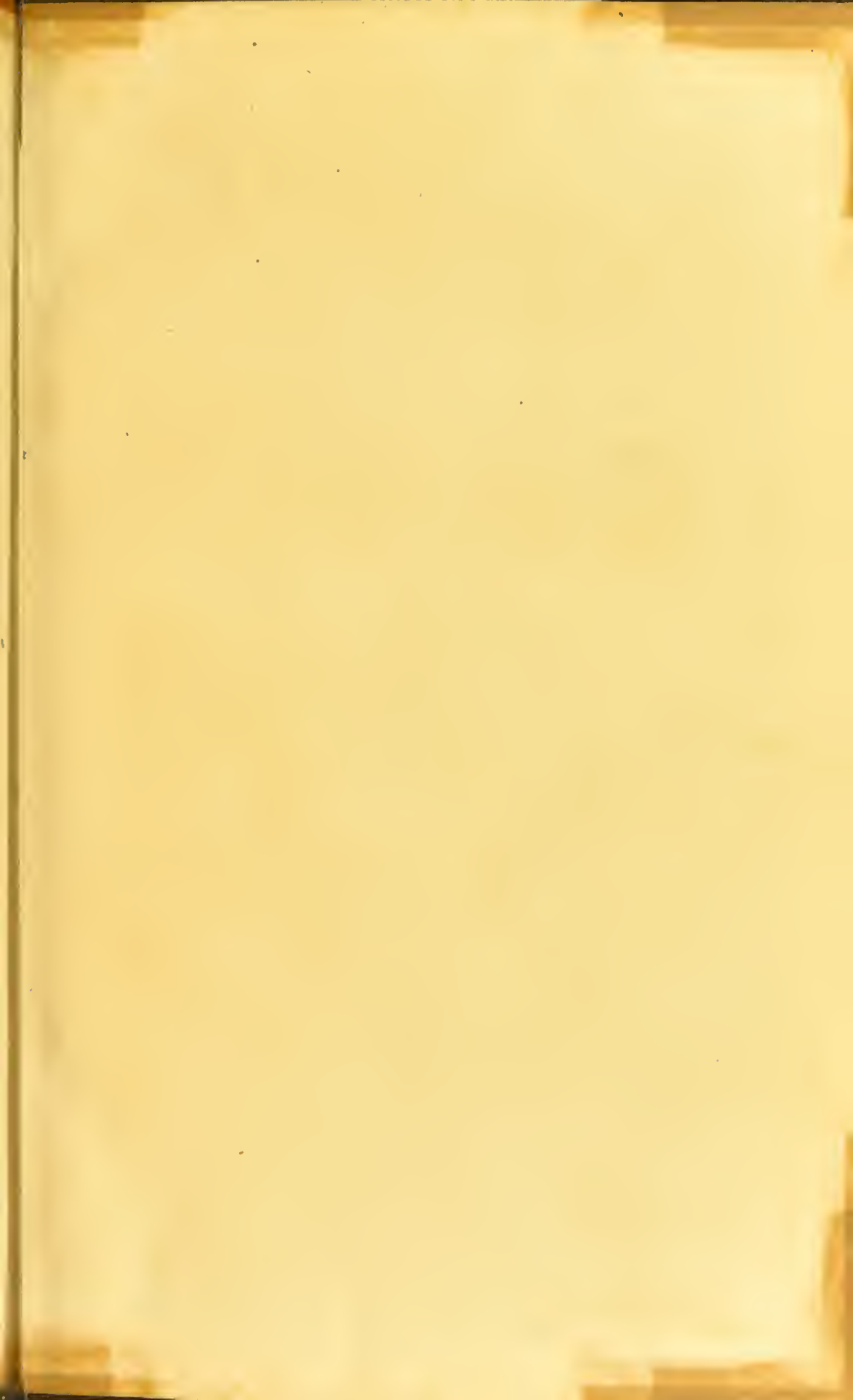


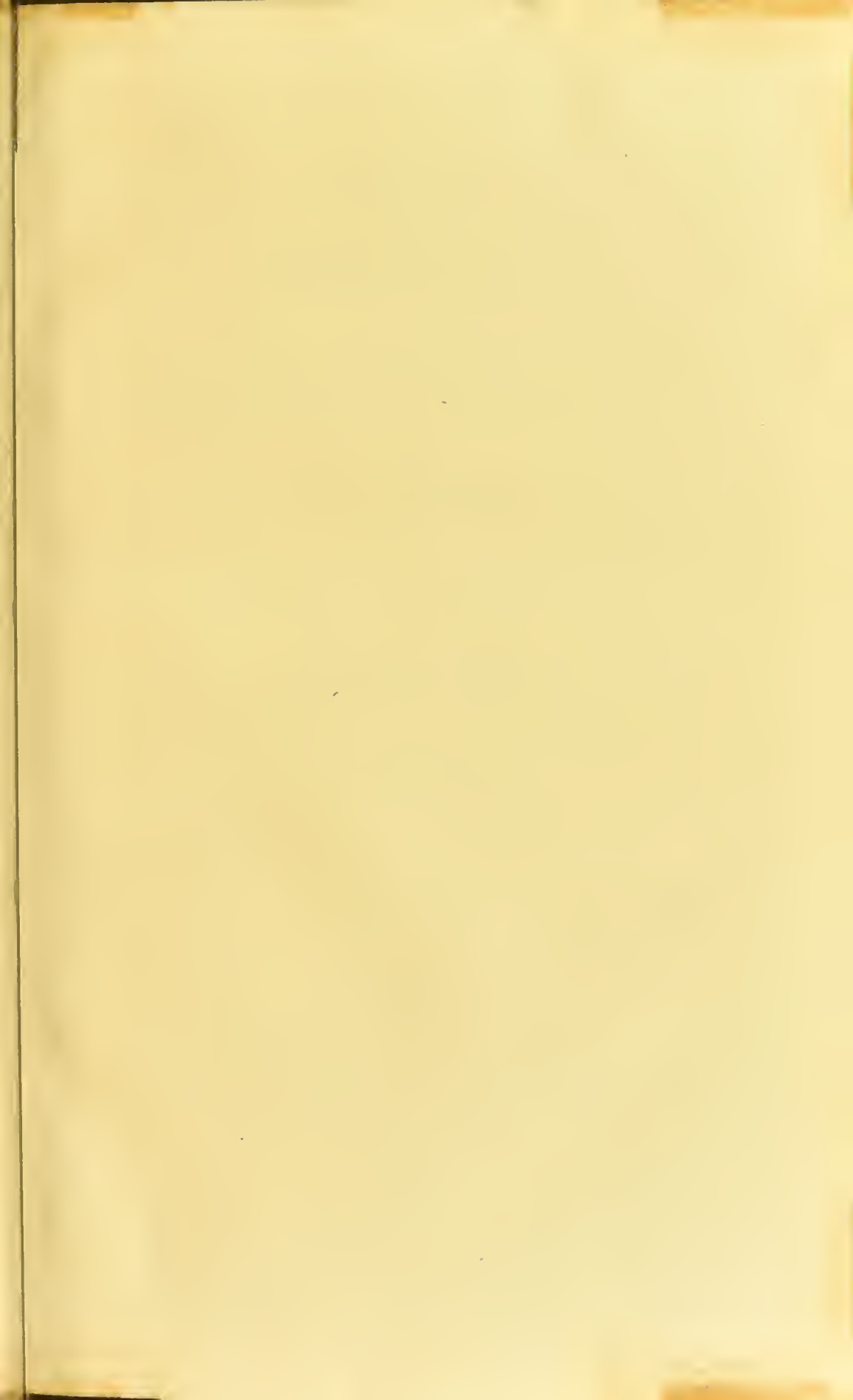


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A
COLLECTION
OF
TREATISES
ON
THE EFFECTS OF
SOL-LUNAR INFLUENCE
IN
FEVERS;
WITH
AN IMPROVED METHOD OF
CURING THEM.

By FRANCIS BALFOUR, M.D.
FIRST MEMBER OF THE MEDICAL BOARD
IN BENGAL.

Donec tu quoque, Phæbe, latebis.

THIRD EDITION.

CUPAR:

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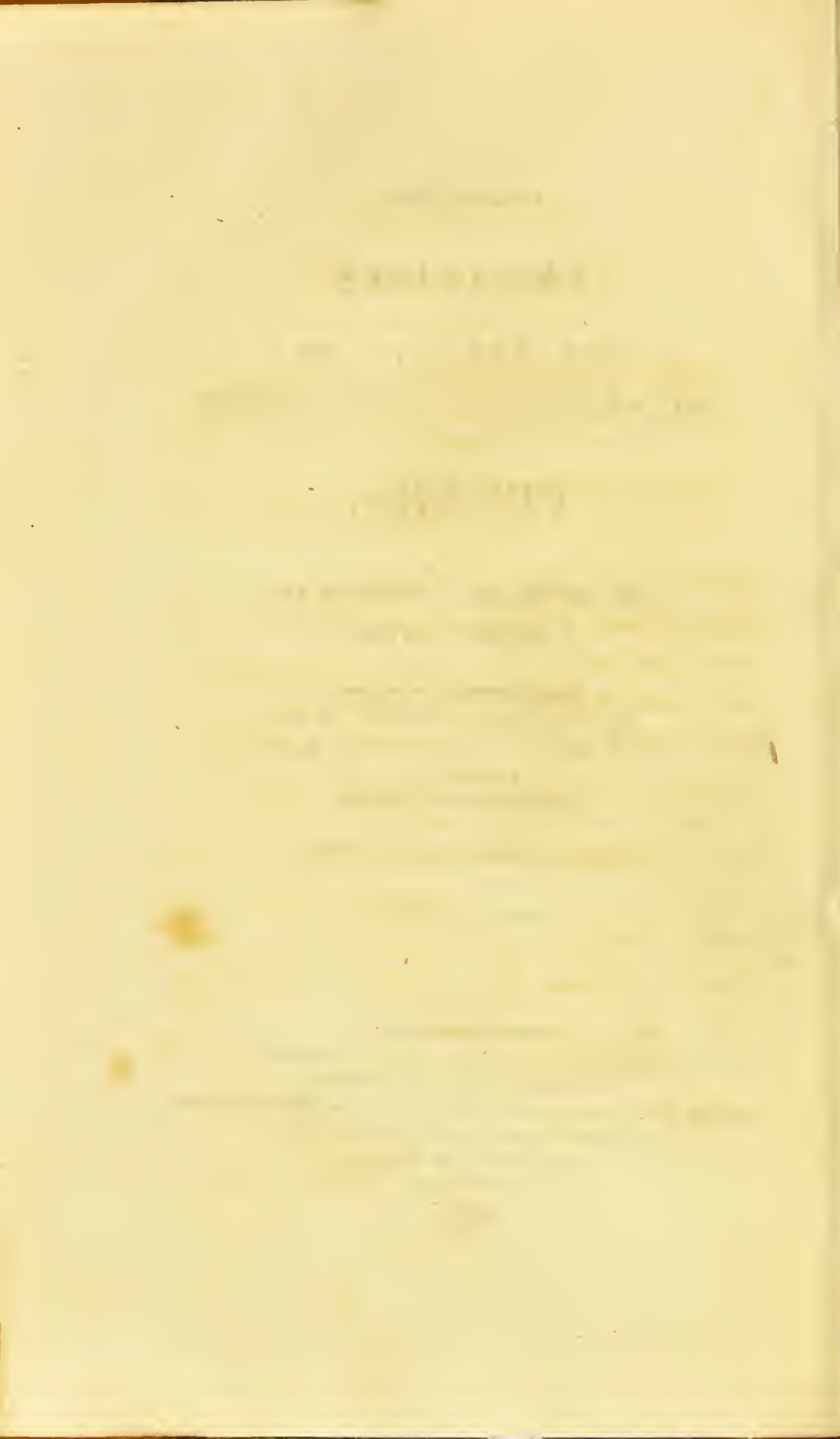
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LONDON.

1815.



TO THE HONOURABLE

THE DIRECTORS

OF

THE EAST INDIA COMPANY,

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HONOURABLE SIRS,

HONOURABLE SIRS,

My original Collection of Treatises on Sol-lunar Influence comprehended the result of all my observations and reflections on that subject during a period of 36 years, spent in India, in the service of the Honourable East India Company.

Having annexed to that original work a SEVENTH TREATISE, containing Matter Defensive and farther Explanatory of my System, I have now the honour to present it under the title of a THIRD EDITION; requesting that it may be received as a testimony of my continued zeal and attachment.

Honourable Sirs,

I have the honour to be

With high respect,

Your most obedient,

And faithful servant,

FRANCIS BALFOUR.

FERNEY, 15th February, 1815.

PREFACE

TO THE THIRD EDITION.

HAVING occasion, at this time, to hail the appearance of an animated and instructive new work, on the Influence of Tropical Climates, &c. by James Johnson, Esq. Surgeon in the Navy, a declared convert to the belief of Sol-lunar Influence, I now begin to suffer less anxiety for the fate of this Theory.—My own feeble exertions, however, to support it, are not as yet entirely suspended; and the Publication of this THIRD EDITION has been directed towards the following objects:

- I.—*To point out to the Editor of the Edinburgh Quarterly Review, Vol. V. March 1812, several very gross mistatements of Matter of Fact, in his report on my work, highly injurious to its character.*
- II.—*To add to the ample proofs of Sol-lunar Influence, already obtained in Asia and other quarters of the globe, some decisive evidences of its effects in EUROPE.—And,*
- III.—*To attempt an explanation of the peculiar nature of the 4th day in Fevers; which has so much puzzled Physicians, both ancient and modern.*

These Objects have been attempted in a Seventh Treatise, which now appears at the end of this Volume.

CORRIGENDA.

CORRIGENDA.

- 1.—In page 11, line 21 from top, after the words “ 28 days” insert, “ The Lunar Periods and Inter-lunar Intervals
“ are represented by the Breckated Divisions of the up-
“ permost line of the Diagram, and distinguished by
“ their respective Titles placed above them ; and the
“ days included in each of those Divisions of the upper-
“ most line are represented by the Breckated Divisions
“ of the line under it, distinguished from each other by
“ numeral letters placed above them.”
- 2.—In page 11, line 15 from the bottom, after the word
“ Second Treatise” insert “ pages 69, 70.”
- 3.—In page 11, line 13 from bottom, the words “ *Medical*
“ *Day*” should have been printed in Italics.
- 4.—In page 12, the Running Title at top should be “ *Notices*
“ *respecting the New Diagram.*”
- 5.—In page 12, after the 3d Paragraph of NOTICES, re-
specting the Diagram, insert the following as a 4th, thus,
4. As Table I. and Table II. referred to in the original
Treatises, to illustrate the Forms of Perfect and Imper-
fect Types, were rendered unnecessary by the NEW
DIAGRAM :—These references may now be erased and
transferred to the DIAGRAM.
- 6.—In page ii. of the MEMORIAL, line 5 from bottom, for
“ 1739” read “ 1784.”
- 7.—In page 358, line 19 from top, for “ 29” read “ 46.”
- 8.—In page 365, line 6 from top, for “ which fever was im-
“ mediately adjoined,—” read “ to which fever was
“ immediately adjoined,—”

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REIGN OF

CHARLES THE FIRST

IN WHICH ARE CONTAINED
THE
MOST IMPORTANT AND INTERESTING
CIRCUMSTANCES OF HIS REIGN
FROM HIS MARRIAGE TO HIS DEATH

BY
JOHN BURNET

IN TWO VOLUMES

LONDON
Printed by J. Sturges, in Pall-mall
1724

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NOTICES RESPECTING THE NEW DIAGRAM.

Plate I. page 96.

1. THIS Diagram is constructed for the purpose of conveying a distinct and comprehensive idea of the whole system of the Sol-lunar theory; by exhibiting, at one view, the coincidence of the Paroxysms and Remissions of Fevers, with the periodical Risings and Fallings of Sol-lunar Force,—exemplified in the common forms of *Perfect Types*, or common continued Fevers; and consequently to point out the true origin of all the different Types, Periods, Durations, Crises, Relapses, Metastases, and other periodical changes to which they are subject. A reference, therefore, to this Figure, is earnestly recommended to the reader before he engage further in the perusal of the work; and first of all it is necessary to call his attention to the following notices, respecting *the scale of Time*, which were accidentally omitted in the description of the Diagram in its proper place, (p. 93.)

A *Medical Month*, according to this system, is supposed to commence 3 days and a half before the *change or full* of the moon; and always to comprehend in succession, a lunar period, an interlunar interval, a 2d lunar period, and a 2d interlunar interval, each containing 7 days; and thus to include in all 28 days.—

The *scale* of this Diagram, commencing 3 days and a half before the *change*, comprehends only a lunar period, an interlunar interval, and a 2d lunar period; and includes only 21 days.

Respecting the *Medical day* of this system it is now to be particularly remembered, that the *limits* and *divisions* allotted to it, by this scale, are considerably different from those originally assigned to it in the beginning of the second Treatise; which will therefore require to be corrected accordingly.

By this alteration the *limits* of the medical day now extend from 3 in the morning to the same hour next day; and the *divisions* of it are adjusted in the following manner:

The 1st extends from 3 in the morning to 9 of the forenoon, & constitutes the *matutine*, or *morning inter-meridional interval*.

The 2d extends from 9 of the forenoon to 3 of the afternoon, and constitutes the *diurno-meridional period*.

The 3d extends from 3 of the afternoon to 9 in the evening, and constitutes the *vespertine*, or *evening inter-meridional interval*.

The 4th extends from 9 in the evening to 3 in the morning, and constitutes the *nocturno-meridional period*. And so with all the rest from left to right.

To render the above defined distinctions of *intervals* and *periods* more obvious, their respective initials, I and P, are placed above them : and their relative positions distinguish, at once, without any other sign, the half of the day to which they belong whether diurnal or nocturnal.

The points of interrogation placed in the two empty brackets allotted for forms of the 3d and 4th genus of perfect types, indicate that their different species have not been accurately observed and ascertained.

2. From this Diagram, representing directly the forms of *perfect types*, we obtain also, by a very simple expedient, an exact illustration of the coincidence of the paroxysms and remissions of fevers of *imperfect types*, with the fluctuations of sol-lunar influence. Nothing more is required than to cover with small bits of paper, or with the points of the fingers, the oval figures representing the paroxysms, which, in any particular case, have not occurred in conformity with the uninterrupted succession observed in *types* that are *perfect* : And by this expedient, a Second Diagram for the illustration of *imperfect types* is rendered unnecessary.

3. Respecting the *origin* of different types, it is certainly to be explained upon the principles of this system, in a more simple and consistent manner than on any other theory that has been as yet proposed. Is it not sufficient to say, that, in *types* that are *perfect*, the febrile state is, by its peculiar nature, excitable to paroxysms by every succeeding increase of sol-lunar force ; and that, in *types imperfect*, the febrile state, owing to a different constitution, is not excitable to paroxysms but at greater intervals of time ?

To these observations I have now only to add, that, in bringing forward the PRESENT EDITION of this Collection, I have endeavoured to preserve, strictly, the chronology of every circumstance relating to the first publication of these Treatises : and it was likewise my wish not to alter, in any respect, the identity of their original form. In the Analysis, however, required in the Second Treatise, for the solution of Problem II, I found myself under the necessity of deviating materially from this rule ; not only for the purpose of accomplishing this object more effectually, but to reject, for the present, the disquisitions respecting the *comparative frequency* of certain forms ; which, being founded on observations not sufficiently extensive, were not satisfactory.

TO THE HONOURABLE
THE DIRECTORS
OF
THE EAST INDIA COMPANY,
THE MEMORIAL OF FRANCIS BALFOUR,
SENIOR HEAD SURGEON ON THE
BENGAL ESTABLISHMENT.
May, 1799.

HONOURABLE SIRs,

THE object of this Memorial is to solicit the attention of the Honourable Court of Directors to certain particulars respecting the Author; and although this indulgence may not be due to the suit of an individual, he hopes that the public importance of the subject which it offers for their consideration, may be some apology for the present intrusion.

To remove the apprehensions that naturally arise from the idea of a medical investigation, having assured the Honourable Court that no proposition shall be advanced, no language used, that is not perfectly plain and intelligible, he proceeds to state, that, from the commencement of his service in Bengal, in July 1769, not content with performing barely the portion of official duty committed to his charge, he thought it likewise incumbent on him to communicate, from time to time,

whatever discoveries or improvements might occur to him in the line of his profession. Accordingly,

In the course of 1769 and 1770, having discovered a new and successful mode of curing the Pucca Fever of Bengal, by exhibiting the Peruvian bark in a manner then unknown in that country, he spared no pains to introduce it into general use, and took the first opportunity of publishing it in England, by sending his Manuscript to Sir John Pringle. And although that has been lost, the principal facts and circumstances (1)* were inserted in a subsequent treatise; and, he hopes, are not entirely forgotten in India, nor in Leadenhall Street.

Having perceived, after visiting the different climates of Bengal, that the propositions which he had advanced in his first treatise, respecting the bark, required various restrictions and modifications; particularly, that *purgatives* were to be administered with much greater freedom than he had at first conceived to be necessary (2,) *especially calomel, the peculiar and beneficial effects of which he had now fully ascertained by long experience, and accurate investigation* (3;) and that the exhibiting of this and of every other remedy, required to be regulated by an attention to the revolutions and changes of lunar influence (4,) he communicated these observations to the public in Treatise 1st, printed at Calcutta in 1789. And by drawing the attention of the profession to these different objects, it will appear from the testimonies (5) to which he refers, that it did not fail to attract their notice, and to have an influence on their practice, favourable to its success.

* This and all other references are collected together at the end of the Memorial.

In the interval between 1784 and 1789, part of which time he had charge of the general hospitals of Chunar and Calcutta, having collected and arranged all the information he could obtain on these interesting subjects, he prepared them for the press on his passage to England, and published them in February 1790, in Treatise 2d, containing the whole of this system. And,

In 1791, in order to explain the nature of this system more fully, and render it more complete, he published Treatise 3d ; which being included with the 2d, in one volume, (5) was addressed, in his Dedication, to the Honourable Court of Directors, in the following terms :

“ HONOURABLE SIRs,

“ THE Volume which I have the honour to lay before you, comprehends the experience and reflection of almost twenty years spent in India in the service of the Honourable Company. It proposes a theory which is new, and recommends a practice which I flatter myself, has been attended with more than common success. It aspires to important improvements in medicine, especially in those climates where your authority and commerce extend ; and naturally looks up to you for the support it requires in presuming to instruct.”

To explain more precisely the nature of this work, the author of it, in having contributed to introduce the method of evacuating the intestines, as the first and principal object in the cure of fevers, by pushing on the exhibition of calomel, assisted by other cathartics, with a degree of energy and despatch unknown

in common practice ; and in having discovered and shown in what manner the revolving tendencies of lunar influence may be applied to concur with these medicines, in their immediate operation, and to maintain and secure their beneficial effects by other means, presumes to believe that he has been instrumental in diffusing the knowledge of a successful method of curing fevers, essentially different from the common and prevailing practice of the time at which he communicated this system to the public ; and further, in facilitating the application of these several principles to practice, by reducing them to a small number of precise, positive, distinct rules, ready at hand to direct and decide the young assistant in his conduct, when he first takes charge, and needs them most, he has been anxious to render them substantially useful ; and to furnish a more appropriate form of conveying medical instruction, than is in general to be found in medical books.

But, although the Author has in this volume completed a system which, in all its parts, pretends to improvements of extensive use, he is not so unreasonable as to expect from your Honourable Court a minute enquiry into the foundation of these various pretensions. He wishes solely to draw your attention to a single question, which will naturally present itself on the perusal of this appeal : viz.

“ Has it been confirmed, by the experience of Physicians
 “ and Surgeons in India, and other quarters of the globe, that
 “ the practice recommended by Mr Balfour, in his Treatise
 “ on Fevers, published in 1790*, has really been more suc-

* Copies of this Treatise were presented very soon after its publication, to the Libraries of the University of Edinburgh, of the Royal College of Physicians, and of the Royal Medical Society.

“ ccessful than the practice that generally prevailed previous
 “ to that period ?”

In considering this question, it is necessary to refer to the different medical books that have appeared since the publication of that Treatise ; and, in making this reference, it will not fail to impress and have due weight in forming the decision that is to be given respecting it, that Dr Wade of Bengal, Dr Rush of Philadelphia, Dr Chisholm of Grenada, Mr Bayley of New-York, Mr Bryce surgeon of the Busbridge Indiaman, and Mr Anderson surgeon to a regiment in the West Indies, all of whom adhered to the object of evacuating the intestines as a first principle to direct their conduct, report, with extacy and surprise, their uncommon success ; whilst Dr Jackson and Dr Maclean, who had charge of the hospitals in St Domingo, and who were guided by other ideas respecting the nature of the disease, and were led to a different practice, are obliged to acknowledge and lament their failure and disappointment. These authorities shall be examined separately†.

In a treatise published in 1793, Dr Wade, (A) who acted : under the author of this memorial as his assistant at Banaris part of 1784, and all 1785 and 1786, and acquired, during that time, a competent knowledge of this practice, having afterwards made observations at Chunar, in Bengal, and on board of an Indiaman, on which he officiated as surgeon, on his return to Europe, *proclaims the inefficacy of former practice (7) ; and confirms, by a reference to many cases, the superior suc-*

† In this examination I have been anxious to advance nothing without a particular reference to the language made use of by myself and the authors to whom I have referred.

cess of the evacuating system by means of calomel (8). Dr Wade's acknowledgments on this subject, respecting the author, were expressed in his manuscript sent to the Medical Board of Bengal ; but in his treatise published in England, it is only alluded to by a note (9), in which Mr Balfour's name does not appear.

In a treatise, published by Dr Rush (v) of Philadelphia in 1794, respecting a fever that raged in 1793, he informs us *that being baffled (10) in every attempt to stop the ravages of that fever, by common means, he was at last directed to the system of evacuating the intestines by calomel (11) ;* and having discovered its wonderful effects, is led to express himself in the following language (12) : “ After such a pledge of the
 “ *safety and success of my new medicine, I gave it afterwards*
 “ *with confidence. I communicated the prescription to such*
 “ *of the practitioners as I met with on the streets. I impart-*
 “ *ed it to the College of Physicians on the 3d of September,*
 “ *and endeavoured to remove the fears of my fellow citizens,*
 “ *by assuring them that the disease was no longer incurable.*
 “ *Mr Lewis the lawyer, &c. &c. nine in number, were the first*
 “ *trophies of this new remedy. The credit acquired brought*
 “ *me an immense accession of business ; it still continued to*
 “ *be almost uniformly effectual in all those whom I was able*
 “ *to attend, either in person or by my pupils. Dr Griffiths,*
 “ *Dr Say, Dr Pennington, and my former pupils who had*
 “ *settled in the city ; viz. Dr Leib, Dr Porter, Dr Anan, Dr*
 “ *Woodhouse, and Dr Mease, were amongst the first physicians*
 “ *that adopted it. I can never forget the transport with which*
 “ *Dr Pennington ran across the street, to inform me, a few*
 “ *days after he began to give strong purges, that the disease yield-*

ed to them in every case." Dr Rush then informs us, that the theory of the proximate cause led him to other remedies, and that he did not rely on purging alone to cure the disease. What he has delivered, however, is sufficiently decided respecting the efficacy of this remedy, without any other assistance.

Dr Rush has acknowledged the encouragement he received from the example of Dr Clark and the author (13) of this memorial, to adopt the purging system ; but refers his first determination in its favour to the perusal of an old manuscript written in 1741 (14), recommending evacuations. Respecting this acknowledgment, I have only to remark, that if Dr Rush had referred to the author's treatise of 1784, which is published at full length in Dr Duncan's Medical Commentaries, he might have found the following hints respecting the use of purging, in the yellow fever, and the superior efficacy of calomel as a purge : " This leads me to observe, that I am inclined to suspect that the bilious fever in the West Indies, with the yellow skin, and other terrible symptoms described by authors, are owing to a neglect of plentiful evacuations downwards at the very beginning : for whenever I have met with it here (Bengal) it universally proceeded from this cause ; and, on the other hand, I have never seen it in that form, where proper evacuations were obtained at first." Vide Dr Balfour's first treatise on the influence of the moon on fevers, reprinted at Edinburgh in 1785, for C. Elliot Edinburgh, and G. Robinson, London, by the desire and recommendation of Dr Cullen, page 24. And in page 37th of the same book, Dr Rush might have found, " that calomel, above all other remedies that I am acquainted with, possesses the power of carrying

away the slime and tenacious bile with which the bowels are so apt to become loaded in this country."

And here the author of the memorial cannot help expressing his hopes that Dr Rush, when he is more at leisure, will perceive that the purging system recommended by him in his treatise of 1790, and even that of 1784, *cannot with propriety be classed* with the practice of Sir John Pringle and Dr Clegghorn (15,) who, though deservedly elevated to a rank in medicine to which the author dares not aspire, *were nevertheless far behind in their mode of purging and giving the bark.*

In a treatise (c) published in 1795, on a malignant fever that prevailed at Grenada in 1793 and 1794, Dr Chisholm, in his dedication (16) to the medical gentlemen of his Majesty's navy and army, expresses himself in the following manner :

" GENTLEMEN,

" The uncommon mortality that marked the epidemic which
 " is the subject of the following essay, among his majesty's
 " sea and land forces in the West Indies, during the last eight-
 " een months, and the *total inefficacy* which the *usual remedies*
 " in similar circumstances were found to possess, will, it is
 " presumed, render any attempt to throw light on the nature
 " and cause, and appropriate treatment of so dreadful a ma-
 " lady, at least not unacceptable to the public."

In the body of the treatise we are informed, that, " finding
 " at length the total inefficacy of the usual method recom-
 " mended in treating malignant fevers," he was induced to ad-
 " opt a *new system of practice* (17;) *in prosecuting which he was*

led to a free use of purgatives and calomel (18.) And although calomel seems to have been given to answer another purpose, it could not, however, fail to produce intestinal evacuations, in the quantity and manner in which it was administered. In the sequel of his practice, Dr Chisholm was led to give calomel in much larger quantities than he did at first, conceiving that the principal object was to excite a salivation (19.) And there is no doubt that the introduction of mercury into the system, in these fevers, is not only perfectly safe, but in many cases of local affection indispensibly required. But it is to be observed, 1st, that, even by Dr Chisholm's own account of his practice, the immediate evacuation of the bowels, by large doses of calomel, is the first object he studies to obtain: 2dly, That although the subsequent doses may be continued with another view, they will seldom fail to have the effect of operating downwards from time to time in the course of the disease: and, 3dly, That the salivation to which Dr Chisholm refers the cure of the fever, may itself be ascribed with equal reason to the previous evacuation of the intestines, and be considered as the symptom of a favourable change produced by this preliminary operation.

That the evacuation of the bowels by calomel is the first object that Dr Chisholm endeavours to obtain in his new method, will appear from his own words (20): “ My mode of giving calomel, since the re-appearance of the disease, is “ to give ten grains to an adult patient as soon as possible, after I see him. This generally operates as an *aperient in the degree required*, about an hour or two after it “ has been given. At the end of three hours more, the same “ quantity is given, adding opium, *or not, as the preceding dos-*

“ *es have acted.* In this manner ten grains are given every
 “ three hours, till the salivary glands become affected, which
 “ generally happens in less than 24 hours from the commence-
 “ ment of this treatment. The effects of the medicine given
 “ in this manner are in general these; The patient becomes
 “ calmer, less restless, and less anxious, the skin being softer
 “ and possessed of an agreeable heat, the stomach being per-
 “ fectly retentive, however irritable it might have been before;
 “ and the eyes recovering their former lustre and sensibility.”

Regarding the whole of these effects, it is to be observed, that all of them are the natural and common consequence of an early and effectual evacuation of the bowels, succeeded by an opiate, and therefore are not to be referred solely to the operation of mercury introduced into the circulation, agreeably to the conclusion of Dr Chisholm (21.) At the same time, if it should be confirmed, by future experience, that, in consequence of this treatment, “ it is not at all necessary to
 “ have recourse to bark, or any other medicine whatever,” as he informs us (22,) candour and justice call upon us to declare, that, in pushing the exhibition of calomel so far beyond the common object of evacuation, he will have introduced an improvement in the cure of these diseases, that merits, in a high degree, the attention and gratitude of the public. We cannot, however, leave this question without adding, that neither the general result of Dr Chisholm’s own practice, nor the success of similar experiments by a gentleman in Jamaica, as stated by Dr Jackson, nor the history and termination of certain cases treated by mercury and opium, given to a great extent by Dr Yeates and Mr Maclean, in the General Hospital of Calcutta, appear to confirm Dr Chisholm’s theory. The

exhibiting of calomel, by Mr Maclean, was in all, or in most cases, accompanied with numerous glysters of castor oil, &c. and surely no person, who knows the natural and unavoidable effects of this combination, will pretend to affirm, that his patients were treated without an effectual and regular evacuation of the bowels; and, from the uniform success of the purging system, it is not unreasonable to infer, that the practice of Dr Yeates and Mr Maclean might have been still more fortunate, had it been confined to this object and to this combination alone.

In a treatise (D) published in 1796 by Mr Bayley of New-York, giving an account of an epidemic fever that prevailed in that city in 1795, he endeavours to convey a general idea of the practice he pursued, in the following paragraph (23 :) “ When the patient was placed in a situation as eligible as “ could be procured, *the leading object was to evacuate the stomach and bowels.* If this important object could be effected “ at the first attack, it was often found to cut short the course “ of the disease; and when it did not do this, it for the most “ part rendered the subsequent symptoms so moderate, that “ they more readily yielded to the means of art.” And although Mr Bayley seems to be unacquainted with the superior effects and advantages of a vigorous use of calomel employed as a purgative, yet the principles of his practice throughout the disease coincide very closely with those of the evacuating system, and give it confirmation.

In a treatise published in 1796 by Mr James Bryce (E,) formerly surgeon of the Busbridge Indiaman, we are furnished with the history of a malignant fever, which Mr Bryce de-

nominated the yellow fever. In this history we are informed, that although the fever was of a more malignant nature than any he had ever seen before, he nevertheless lost only 3 patients out of 300 cases (24.) *And he ascribes his uncommon and wonderful success to his having rejected the usual modes (25) of treating such fevers by mild purgatives and bark, and to his having adopted a new system, by which the whole of his attention was instantly directed towards the effectual and constant evacuation of the bowels (26) by large and frequently repeated doses of calomel, either by itself or with other purgatives. "I had soon the satisfaction," he says, "to find that by means "of the most drastic purgatives (27,) provided early recourse "was had to them, I had acquired complete controul over "this cruel distemper, even in its most formidable attacks."*

In a treatise published in 1798 by Mr James Anderson (F,) late surgeon to the 6th Regiment, serving in the West Indies, we are informed (28) that, "after many years residence in "the West Indies, during which time he had many opportunities of seeing the disease in all its forms; and of trying, "and seeing tried by others, the different modes of practice which had been most recommended; there is, in his opinion, "but one great object to be held in view in almost every fever of the West Indies, viz. proper evacuations downwards " (29)," to be effected by administering repeated doses of calomel (30) with antimonial powder; and, as a proof of his extraordinary success, he adds, that in 1796, previous to which period he had found the *common modes of cure very ineffectual* (31,) by confiding in his *newly adopted mode of giving calomel* he lost only a single patient out of about seventy (32), who were all suddenly seized with the fever on board of a transport in very unfavourable weather (33.)

Dr Wright physician to the army, and Director to the military hospitals at Barbadoes, in a very interesting report signed by all the gentlemen of the medical staff on that Island, *confirms the use of the purging system in fevers, in a very decisive manner*; as appears by the following extract taken from the appendix to Dr Currie's Medical Reports on the effects of water, published in 1797, p. 51. 53.

"The first intention was directed to a speedy evacuation of morbid matter : The second intention was to prevent the secretion and accumulation of more : The third to relieve urgent symptoms : And the fourth to relieve the mischief already done to the system."

"First, early and brisk purging was put in practice. After some trials of cathartics, we had recourse to large doses of calomel, repeated at proper intervals, until a plentiful discharge by stool was obtained. By this means the vomiting instead of being increased, was generally abated, and at last subdued. By calomel, the pores of the skin were opened, a resolution of the fever was brought about, and the patient happily recovered, &c."

We are now led to consider the mode of cure recommended by Dr Jackson, and Dr Maclean, who had charge of the sick on the island of St Domingo. And as it is generally known and acknowledged by these gentlemen, that it was by no means successful, it is incumbent on the author of this memorial to shew, that the practice which they pursued was *not the same* that has been so confidently recommended by him, and the different authors to whom he has referred.

In the treatise published in 1797 by Dr Maclean (G,) we

learn, that this ingenious author was not unacquainted with the system of evacuating by calomel. But there is reason to suspect that it was never prosecuted with sufficient steadiness and confidence, to ensure that success which it is capable of producing: In short, never obtained a full trial, either by Dr Maclean himself, or the gentlemen who did duty in hospitals at Port-au-Prince.

With respect to the practice of these gentlemen, it is to be collected, by referring to Dr Maclean's enquiry, from page 115 to 164; and it does not appear to me from that account, that calomel was administered properly, and supported by the bark and other means, in such a manner as to give it a fair chance of being successful: but, on the contrary, that it was interrupted and counteracted by other theories and other remedies.

With respect to the practice that Dr Maclean himself pursued on his first arrival, we have it from his own authority, that it was unsuccessful (34) and fluctuating (35); and, with regard to the practice that he adopted latterly (36), after many fluctuations of opinion, and of which he ventures only to report, "that it succeeded happily in many instances" (37), it is a practice in which neither intestinal evacuations, nor bark, have the place or confidence assigned to them in the system recommended by the author of this memorial.

In Dr Jackson's (11) outline of the fevers of the West Indies, published in 1798, it appears that he is sufficiently acquainted with the praise bestowed, by different authors of the East and West Indies, on the plan of evacuating by calomel, supported by bark and other remedies; and admits, that in

visible types, the purpose will often be effected ; but that, in the more violent forms, when the dawnings of remission were obscure, it was found to be uncommonly unsuccessful. The following extract contains Dr Jackson's own words (38) :
“ The more general method of cure in the endemic fever of
“ tropical climates, hinges on procuring remissions by means
“ of purgatives ; the known virtues of the bark are sufficient
“ to complete the rest. Salts with emetic tartar, jalap with
“ calomel, or calomel with antimonial powder, are the com-
“ mon forms by which that object is attempted to be accom-
“ plished. In fevers fundamentally of type, the purpose will
“ often be effected : but in fevers so concentrated, that the
“ dawnings of remission are scarcely perceptible, the expecta-
“ tions of success are uncertain, &c. The second part of
“ this method of cure consists in stimulating by wine, opium,
“ camphire, and blisters, or in preventing recurrence, by large
“ and often repeated doses of bark. The view is good ; but it is
“ a view so much relative to the condition of existing circum-
“ stances, as to require accurate discernment in the applica-
“ tion : this perhaps has been deficient ; for the mortality of
“ the more violent forms of fever under this mode of treat-
“ ment, is found to be uncommonly great.”

With respect to this paragraph, we have to observe, 1st, That although it be true that the method of giving calomel, supported by bark, which is nearly the practice of the author of this memorial, and many others, depends much, as Dr Jackson supposes, on a correct view of relative circumstances, and an accurate discernment in the application of it, yet the process is now rendered sufficiently distinct and easy, by a steady and confident adherence to the precise rules that have been

laid down by those who know its efficacy and success : and, 2dly, that, as far as we have been able to collect from Dr Jackson's account of his theory and practice, we cannot perceive that he ever gave it a full trial ; or that he ever prosecuted or adhered to these rules with sufficient steadiness or confidence ; and, therefore, that the report which he makes of the great mortality that prevailed in certain forms of the disorder, may reasonably be referred, as he very candidly admits, to a defect in its application.

From his theory and practical instructions, it will further appear, that he had very different objects (39,) and very different remedies, in view ; and, from his cases of endemic fever of a more violent form (40), which are the only specimens we have of his practice in fevers of this denomination, and of which 9 out of 18 proved fatal ; we discover that, although calomel was used as a purge, yet this was done in such a manner as to render the practice inefficient, and essentially different from that which is recommended to the public by the author of this memorial in his treatise of 1790 ; and which, since that time, has been confirmed by the united experience of the respectable authorities to whom he has referred *.

The whole of these testimonies, concurring to prove that the practice recommended by the author in his treatise of 1790, *has been uniformly more successful than the practice which generally prevailed before that time ; and that there is no proof of its having ever been unsuccessful, when properly administered,*

* These were all the late publications, that the author was able to procure for this Review. Dr Hamilton's discovery of the use of purgatives in fevers had not as yet appeared. It was not published, I believe, till 1802 or -3.

furnish an answer to the question under consideration, that is highly favourable to this appeal, and will not fail to make a due impression.

Of this system, the practical rules which were originally formed by the author for the general hospital of Calcutta, then under his charge, and form a part of his treatise of 1790 (41), are now submitted to the Honourable Court, in the same form and order in which they appear in that work; and he hopes that they will be found to be well calculated for the intended purpose of deciding and directing the conduct of the young assistant, when he first takes charge of his duty, and needs them most †.

With respect to these practical rules, it is necessary to premise, that the cure is supposed to commence from the first appearance or rather suspicion of the disease; because this being a consideration more essential perhaps than any other, not only for preserving the lives of soldiers and sailors, but to secure at all times the greatest number fit for doing duty, and supposed, on this account, to be constantly held in view by military and naval officers, as well as surgeons, it is presumed that patients will seldom or never come into an hospital in the advanced stage of a dangerous fever. When this unfortunately happens, certain deviations from these rules will of course be required; which, however, will be easily deduced from the general principles.

To convey an adequate idea of the principles and reasoning

† As an immediate reference can be made to Treatise IIId, these rules are now omitted in this place.

from which the preceding rules have been deduced, and of the study and labour that has been bestowed in compressing into so small a compass the practical application of so extensive a system, the author must refer to the original itself, his treatise of 1790. Being formed more than ten years ago, they might no doubt receive, from later observation and experience, many alterations and improvements *; and how far positive rules of this kind, carefully altered and amended from time to time, by the different Medical Boards that have been instituted in the service of his Majesty, and of the Honourable Company, and issued by their *authority* to the different medical establishments under their direction, might be useful and beneficial, is a question that may not be unworthy of public attention. The author has never met with any regulations of this description either in India or in Europe: But, to supply the inexperienced and diffident with distinct rules for their conduct, at the moment they are most liable to hesitate and err; and, on the other hand, to confine the speculative and rash to an approved and established system of practice, from which they dare not deviate, are surely objects of some importance to the state.

Thus far the author of this memorial has been solely anxious to establish *the success and utility of the practical part* of the system which he has ventured to recommend to the public: knowing well that it is by success and utility only, that it can hope to merit the approbation and notice of the Honourable Court. THE THEORY OF THAT PARTICULAR STATE OF THE

* The beneficial effects of the mode of fumigating with nitric, &c. instituted by Dr Carmichael Smith, seems to receive confirmation daily from many trials and proofs, and to merit highly public encouragement and support.

BODY, that has led him to the choice of appropriate remedies, and the DOCTRINE OF LUNAR INFLUENCE, which has been employed to direct the application of these remedies with propriety and advantage, have been carefully separated and kept at a distance from the principal question. But that question being now discussed, it is of importance to the object of this memorial, to make a few observations respecting each of these Theories.

With regard to THE THEORY OF THAT PARTICULAR STATE OF THE BODY IN FEVERS †, *which led the author to the choice of certain remedies, in justice to himself, it is necessary to say, that it is founded on an accurate knowledge of the state of the bowels, and their contents, in these disorders ; obtained, not from the perusal of medical books, nor the random anticipations of conjecture, but from an assiduous and conscientious examination of appearances in the closets of the sick, during a long tract of years.—It is by this means alone, that the practitioner can possibly acquire a knowledge of those remedies that are most efficacious and proper, or learn to prescribe them with judgment and success : and he that will not submit to these loathsome and dangerous investigations, must blunder in the dark ; and fail essentially in the performance of his duty.*

Respecting the advantages that have been derived, or may still be expected, from the labour that the author has bestowed in discovering and applying the *laws of sol-lunar influence to the improvement of medicine*, he must now refer to his IVth Treatise, published in Bengal in 1794. By a reference to the

† Treatise on putrid intestinal, pages 73, 74, 75, 76, 111, 112, 113, 114.

preface of this treatise, page 189, it will be perceived, that, soon after his return to Bengal in 1792, in order to establish more firmly the truth of this doctrine, and to render his system more complete in other respects, he undertook to form it anew into three distinct volumes; and, in the publication of the first *, to which he now refers, he has finished a very important part of his task.

In this volume he has established a proof of the reality of sol-lunar influence in the fevers of India, on evidence that cannot possibly be contraverted or denied; and having likewise confirmed, upon the concurring testimony of many medical gentlemen of experience and ability, *that the knowledge of this influence is employed by them successfully in directing their practice* (42), there can be no doubt that essential advantages have been already obtained, by applying this doctrine to the practice of medicine; and that those who are ignorant of this principle, or inattentive to it, are liable to essential mistakes, which they might otherwise avoid.

The extent and utility of the objects which the author has pledged himself to accomplish in the subsequent volumes of this new work, are explained in his preface to the first (43), and will not escape the notice of the Honourable Court.

And although these are not as yet sufficiently advanced to be laid before them at this time, yet he is not without hopes that the short anticipation of these discoveries, which he has attempted to communicate in the *Postscript* † to the first vo-

* Vide explanatory note in front of the Dedication to Treatise IVth.

† Vide the postscript to Treatise IV. p. 302.

lume, and to illustrate by two synoptical tables, will suffice to convey a distinct idea of the extensive reformation which they promise to produce in the history and science of medicine. The principal phaenomena of fevers, their various types, durations, and crises ; in short, the whole mysterious nature of their paroxysms and periodical changes, which at this moment involves every other medical system to be met with in books or public lectures, in confusion and absurdity, is rendered perfectly intelligible and consistent, *by doing nothing more than taking the trouble to mark and record with a steady and continued attention, the uniform coincidence of these phaenomena in certain states of debility and disease, with the flowing and ebbing of the sea.*

In these particular states or dispositions, comprehending under them many varieties and degrees, and constituting what may be denominated, under one general term, *The paroxysmal disposition*, the paroxysms that are excited almost invariably occur, either in coincidence with every succeeding tide, forming *types*, which on this account we have denominated *perfect* ; or, with every 2d, 3d, 4th, or other tides more remote, forming *types* which, on account of this interrupted connection, we have denominated *imperfect*. For an illustration and proof of this proposition, we refer to Table I. at the end of the Memorial, *which explains the whole mystery of types : and all the diversities of continued remitting and intermitting fevers.*

With respect to the *duration* of fevers, it is to be premised, 1st, That the paroxysms of fevers most frequently shew themselves with the spring tides, and as these rise to their greatest height, become at the same time most violent and obstinate.

2dly, That, on the other hand, they tend no less invariably to subside and terminate during the neaps. And, 3dly, That although this tendency to subside and terminate during the tides of the neaps, be evident and unquestionable, yet it is well established, in every kind of typhus, where limited durations are best observed, that the peculiar state or disposition of the body required to co-operate with the neap tides in the production of a *final crisis*, seldom or never takes place in less than 4 days; and, on the other hand, is rarely protracted beyond 21.

Fevers of all the different types that are produced in the manner described in the first table, are limited to forms of *various duration* by the remarkable remission which takes place in the power of sol-lunar influence on the commencement of the neaps, and which brings them, at these junctures, to a termination or *crisis*, whenever the state of the body is sufficiently disposed to concur in this event (44.)

The operation of this law is explained in Table II. which exhibiting at one view, all the more common variations that are produced *in the duration of fevers of a perfect type*, conveys also a clear and distinct idea of all the different variations that may likewise be produced by the same law *in the duration of types that are imperfect*; and unfolds, at one simple view, the dark, and once impenetrable secret of the crisis of fevers; and of all the *peculiar diversities that have appeared in their duration since the world began*.

As no paroxysms of fever ever pass the commencement of the neaps, without some sudden and conspicuous abatement

or remission in the degree of their violence, such as does not occur at other times, those remarkable changes are apt to be considered as critical and final solutions of these fevers; and we have reason to believe, that they have actually, in most cases, suggested the idea of their different durations. Upon this ground, such varieties only are exhibited in the table as are supposed to be limited, and formed by the abatement of their violence on the first day of the neaps in which they terminate. At the same time, we wish it to be understood, that the paroxysms of fevers do not always cease or terminate all at once on the commencement of the neaps, but often continue to return in a more moderate degree, and at last subside and disappear, in the course of it, gradually and imperceptibly (45.)

It is further to be observed, with respect to these various forms of duration, that some are supposed to occur more frequently than others. But, although some accurate observations have been made on this subject by Dr De Haen of Vienna, and Dr Rutty (46) of Dublin, and some other physicians, the detail is not sufficiently extensive for any general conclusion. In India, the Author has observed fevers terminating in all these forms indiscriminately; but being perfectly satisfied as to the operation of the general law, took no pains to ascertain the respective proportion of each with precision; and entertains no doubt, that, whatever may be the result of future observation on this point, it will be perfectly consistent with these principles.

Respecting the *postponings* to be observed in the attacks of paroxysm, in every case where the natural tendency of the pa-

roxyssmal state is not disturbed by any particular cause, these, as far as the author's observation extends, are likewise invariably coincident with the postponings of the tides. It is true, that paroxysms are not unfrequently observed to *anticipate*; but this we are generally able to ascribe to some occasional increase of the natural propensity to paroxysm, produced by improper management, or to some other obvious occasional cause: And when fevers are properly treated, when the bowels are kept sufficiently open, and the patient is managed with judgment and attention, the natural and constant tendency of paroxysms, like the tides of the sea, is to postpone their attacks at each succeeding return.

This is a general view of the doctrines that arise in explaining the effects of sol-lunar influence. And, if certain partial deviations should occur, that may appear to be inconsistent, or not well explained by the general law, it is to be considered, that this influence is constantly liable to be increased or diminished by the changing positions of the planetary system; that its effects may be modified by acting on the constitutions of different latitudes and climates, and of individuals differing from each other in their nature and tendency; and that these again may be diversified by all the various accidents to which the human frame is liable from other causes. All this being considered, it is far more consistent with philosophy and candour, to search for an explanation for such deviations, from amongst circumstances such as these, than to reject a natural and connected train of established facts, upon the frivolous and impudent pretence of some apparent inconsistency, or from the difficulty of explaining a single phenomenon. The different types and durations of the fevers of Europe, as described

by Hippocrates, and almost every medical author since his time, resemble, in every respect, the types and durations which in Bengal are invariably connected with the fluctuation of the tides, in the manner we have stated ; and because, in the histories of the fevers of Europe, the actual coincidence of their different forms with these periodical revolutions, owing to a general and deplorable neglect of accurate chronology in clinical journals, has been minutely observed and recorded only in a few instances (47), will the acute and intelligent student of medicine be induced to believe, that the resembling febrile forms of Asia and Europe are produced by two distinct laws of nature, totally unconnected with each other ? And will he search for other laws to account for similar forms in Africa and America ?

In contemplating the whole of this doctrine, we cannot doubt that a theory so simple and connected, in all its parts, with the known and established laws and motions of the universe, will at length prevail, and be able to expel from the schools of medicine, the deplorable expedient of supporting, in contempt of every rule of improved reason, the *VIS MEDICATRIX NATURAE* :—an idol erected on air, in times of ignorance, to explain *the natural revolutions and changes produced in the paroxysmal dispositions of the human frame, by the revolving influence of the sun and moon* ; of which being totally inattentive and ignorant, they were not able to give any rational account.

Thus far the Author has endeavoured to explain the effects of this doctrine upon the theory and practice of medicine. In applying it to the reformation of *medical language*, he does not

hesitate to promise, that all the present systems of medical arrangement and definition, erected on imperfect and erroneous views of nature, and every where creating distinctions and characters from variable and fleeting accidents, assumed as attributes essential and permanent ; and, still more, those arrangements that have been woven and manufactured wholly in the brain, must at last give way to one that is formed and established on the broad and firm basis of an obvious and universal law, acting on the whole system of nature, and giving its own form and pressure to all its parts :—giving to diseases shapes and attributes that are essential and inseparable ; and which cannot be omitted *in forming correct and well defined characters for the purposes of language and science.* This is the great and important object of all medical arrangement ; for which the inimitable Cullen laboured with so much zeal and success ; and for which he has received so much unmerited abuse.

With regard to the *whole* of this medical system, it is of consequence to the Author of this Memorial, that his Honourable Masters, whom he has served for 30 years, should perfectly understand that his labour and study have not been confined merely to the consideration of a few detached diseases, but that these improvements are immediately applicable, in practice as well as in theory, to the treatment of the whole class of febrile disorders, from the plague down to the simple intermittent ; a class which comprehends not less than three-fourths of all the diseases to which the human frame is subject, and those by far the most destructive and formidable. And if so small a volume as the Treatise of 1790, in which the whole system is contained, should appear to be inadequate to the

explanation of so large a portion of the medical art, he trusts that those who will do him the favour to examine it with attention, will be able to discover, that to have swollen it into many volumes of an ostentatious size, would have been far more easy than to compress it into the diminutive form which it assumes at present. And, *Finally*,

If it shall now appear to your Honourable Court, from this Memorial, that the Author of it has actually contributed, not by accidental desultory effusions, but by a regular and connected train of observation and study, continued through a long tract of years, to introduce and diffuse a system of practice in fevers found to be more successful than that which prevailed before ; and that he is still exerting himself to render it more complete, he will have no occasion to make an apology for this address ; but may entertain hopes that it will be received with indulgence and favour.

☞ Whilst the Author of this Memorial is anxious to establish his own pretensions to particular improvements that form the distinguishing character of this system, he is at the same time desirous to claim nothing more than the merit of having *contributed* towards these, with some of his predecessors and cotemporaries, who have likewise wrote on these particular points, tending to improve them. To enumerate all, and to assign to each his respective proportion, would be a task of considerable difficulty and labour ; and is not required in this place. Yet he will venture to say, without any apprehension of giving offence, that Dr Clark of Newcastle, formerly in the service of the Honourable Company, has, by various publica-

tions, contributed to introduce a method of treating the diseases of warm climates, that has been extensively and singularly useful. It consists in a combination of constituent parts, resembling, in many respects, that of the Author; but, perhaps, rather less urgent and vigorous in the predicament of purging. At a very early period of his practice, Dr Clark discovered the real power and use of the peruvian bark; and, with Dr Millar *, Dr Robertson †, and one or two more, took an early opportunity of recommending it earnestly to the public.

BOOKS REFERRED TO IN THE MEMORIAL.

A Manuscript by Francis Balfour, M. D. on the safety and advantage of giving the Peruvian Bark in the paroxysm of fevers, as well as their remissions, written in Bengal in 1769 and 1770, and carried home to Sir John Pringle by Sir Archibald Campbell. Of this Treatise, the principal facts and conclusions are preserved in Treatise II.

I. A Treatise on the Influence of the Moon in Fevers, by Francis Balfour, M. D. first printed at Calcutta in 1784, and afterwards re printed at Edinburgh in 1785, for C. Elliot, and Robinson, London. To which last the Author refers in this Memorial.

II. A Treatise on Putrid Intestinal Remitting Fevers, &c. by Francis Balfour, M. D. printed at Edinburgh in 1790. In

* Dr John Millar on the prevailing diseases of Great Britain.

† Dr John Robertson, physician to Greenwich hospital, and author of several valuable treatises on Fevers.

this Treatise the different *periods* of sol-lunar influence are fully explained.

III. A Treatise on the *Action* of Sol-lunar influence, &c. by Francis Balfour, M. D. printed at Edinburgh in 1791. *N. B.* Treatise II. explaining the *periods*, and Treatise III. the *action*, or rather the effects of sol-lunar influence, forming together a system on this subject, were bound up in one volume, under the more comprehensive title of a Treatise on Sol-lunar Influence; which is sold by F. Murray, NO. 32, Fleet Street, London.

IV. A Treatise on Sol-lunar Influence in Fevers, &c. by Francis Balfour, M. D. first published in Bengal the beginning of 1794, and afterwards printed in London in 1795, by H. Murray in Fleet Street.

V. A Treatise on the Barometer, by Francis Balfour, M. D. read before the Asiatic Society at Calcutta, in May 1794, and inserted in the IVth volume of their *Researches*.

A. A Paper on the Prevention and Treatment of the disorders of seamen and soldiers in Bengal, by John Peter Wade, M. D. printed in London in 1793.

B. An account of the Bilious Remitting Yellow Fever, as it appeared in the city of Philadelphia in 1793, by Benjamin Rush, M. D. printed in Edinburgh in 1796, 12mo.

c. An Essay on the Malignant and Pestilential Fever of the West Indies, as it appeared at Grenada in 1793 and 1794, by Dr Chisholm. Printed in London in 1795.

D. An Account of the Epidemic Fever which prevailed in

the city of New York in 1795, by Richard Bayley ; published at New York in 1796.

E. An Account of the Yellow Fever, with a successful method of cure, by Mr James Bryce, surgeon of the Busbridge Indiaman ; published in Edinburgh in 1796.

F. New facts and Observations on the Yellow Fever of the West Indies, by Mr James Anderson, surgeon of his Majesty's 60th Regiment ; published in Edinburgh in 1798.

G. An Enquiry into the Nature and Cause of the great mortality among the troops at St Domingo, by Hector Maclean, M. D. published in London in 1797.

H. An Outline of the History of Fevers, &c. of the West Indies, by Robert Jackson, M. D. published in Edinburgh in 1798.

REFERENCES.

- (1) vide page 33, 34, 35, &c. of Treatise I. by Dr Balfour.
 (2) — 36, 37, 38, 39, 42. ditto
 (3) — 43, 44. ditto
 (4) — 40, &c. ditto
 (5) — NO. 11, 14, 15, 16, 17, 19, 21, }
 25, 20, 28, 29, 30, 31, 37, } of Treatise IV.
 38, 40, 42, 43, 48, 51, 52, } by Dr Balfour.
 53, 54, 56, 57, 62, 63, 64, }

All of which are letters from different medical gentlemen in Bengal, all testifying the existence of sol-lunar influence ; and many of them the advantages of attending to it in practice.

(7) vide page 85, &c. of Treatise (A) by Dr Wade

(8) — 90, &c. ditto

- (9) vide page 45, &c. ditto
- (10) — 140 of Treatise (B) by Dr Rush
- (11) — 143, &c. ditto
- (12) — 144, 145, &c. ditto
- (13) — 142 ditto
- (14) — 141 ditto
- (15) — 142 ditto
- (16) — iv Dedication of Treatise (C) by Dr Chis-
- (17) — 150 of the Treatise. [holm.
- (18) — 152, 153 ditto
- (19) — 171 ditto
- (20) — 172 ditto
- (21) — 173 ditto
- (22) — 173 ditto
- (23) — 99, &c. of Treatise (D) by Mr Bayley
- (24) — 7 of Treatise (E) by Mr Bryce
- (25) — 47, 48 ditto
- (26) — 49, 50, 52, &c. ditto
- (27) — 49 ditto
- (28) — 2 of Treatise (F) by Mr Anderson
- (29) — 19 ditto
- (30) — 28, 29, 30, 31, 32 ditto
- (31) — 14, 15 ditto
- (32) — 31 ditto
- (33) — 27 ditto
- (34) — vii of the Preface to Treatise (G) by Dr
Maclean
- (35) — xi of the Preface ditto
- (36) — 154 of the Treatise by Dr Maclean
- (37) — xi of the Preface ditto

- (38) vide page 291, 292 of Treatise (H) by Dr Jackson.
 (39) — 256 to 298 ditto
 (40) — 129, &c. of Treatise (H) by Dr Jackson,
 (41) — 111 to 131 of Treatise II. by Dr Balfour,
 (42) — (5) Reference.
 (43) — Preface to Treatise IV. by Dr Balfour.
 (44) — Treatise II.
 (45) — ditto
 (46) — A Chronological History of the weather
 and seasons, and of the prevailing diseases in Dublin for forty
 years, by Dr John Rutty, published in Dublin in 1770.
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§ The analysis of Dr Currie's Table, referred to in Treatise VI. was originally annexed to this Memorial, to shew the effects of sol-lunar influence at the Equinoxes. The substance being inserted there, it is unnecessary to repeat it.

PREFACE

¶ In 1800, I returned once more to Bengal ;
and in 1805, having formed into one volume the
Treatises referred to in the preceding Memorial,
with others that had appeared separately at dif-
ferent times, the General Collection was presented
to the Most Noble Marquis Wellesley, then Go-
vernor General, with the following Dedication and
Preface.

TO HIS EXCELLENCY THE MOST NOBLE
RICHARD MARQUIS WELLESLEY,
GOVERNOR-GENERAL, &c. &c.

May it please your Excellency,

I HAVE the honour to lay before you a COLLECTION OF TREATISES relating to the important subject of FEVERS, composed at different periods of my service in India; and I have ventured to prefix a PREFACE, recommending a simple and successful method of treating these disorders, in the military hospitals of Bengal.

The materials of the practical system, delineated in this Preface, are the results of the experience, not of any single individual, but of many ingenious and able practitioners, employed in the treatment of fevers during a long tract of years: and the different modes of suc-

DEDICATION.

cessful practice that now prevail in this country, admitting, generally, the same principles, in various modifications and degrees, seem to differ from each other in this respect only; and more or less to approximate to the system that I have explained.

In delineating this system, I, therefore, ascribe little more to myself than the merit of absolving a duty incumbent on my situation, by the efforts that I have made to accomplish these different objects.

1st. To secure it against the fluctuating innovations of speculative and inexperienced strangers, “by giving it the firmness and stability of a defined and intelligible form;”

2dly. To prepare it for immediate use and application, as a safe conductor to the young assistant surgeon, “by reducing it to a small number of precise and distinct rules;” and,

3dly. To present it as a sure and solid basis for progressive improvement, “by placing

DEDICATION.

“ it on simple truths, only, established by experience.”

Believing that it will appear to your Excellency, that the exertions that I have made must in some degree contribute to these desirable ends, I will not approach with diffidence, soliciting to place the result of my professional labours under your Excellency's protection.

May it please your Excellency,

I have the honour to remain,

With high respect,

Your Excellency's

Most obedient and

Faithful servant,

FRANCIS BALFOUR.

Calcutta,
27th April 1805. }

PREFACE

TO THIS

COLLECTION OF TREATISES.

AS the diseases of Bengal, that are by far the most frequent and most dangerous, both to Europeans and natives, and which therefore constitute the most important object of hospital practice, belong chiefly, and almost wholly to the class of fevers, in which are included dysenteries, &c. my attention has naturally been directed to this subject more than any other; and all the Treatises that are now presented to the public, in this volume, are intimately connected with it. Having conceived the general idea of forming from the scattered materials of these different Treatises, and from additional knowledge acquired since their first publication, a more distinct view of the different objects that unite to form a rational and intelligible system of practice, I framed the design of considering each of these objects fully and separately: and ultimately the nature of their general combination into one system in the following order:—

- I. *The laws of sol-lunar influence.*
- II. *The nature of the febrile state.*
- III. *The effects of purgatives and Peruvian bark.*
- IV. *The use of mercury, as a febrifuge.*
- V. *The combination of these materials into one system.*
- VI. *The remedies employed as auxiliaries to this system.*
- VII. *The reduction of this system to precise practical rules.*
- VIII. *The conclusion.*

I. OF THE LAWS OF SOL-LUNAR INFLUENCE.

In the prosecution of this design, I commenced my undertaking with a general statement of all the knowledge I had acquired, respecting *the laws of sol-lunar influence*; and the sum of my observations on this article, which was delivered to the Asiatic Society, at their meeting on the 7th of July, 1802, and inserted in the VIIIth volume of their *Researches*, constitutes the last Treatise of this collection.

In this Treatise it will be observed that, besides explaining in a more full and distinct manner the nature of the types and durations of fevers, I have acquired, since the publication of those that preceded it, a more precise knowledge of the changes that take place in the force and action of sol-lunar power at different periods of the moon's *parallaxes*; a circumstance of very great importance: because it not only gives us an addition of knowledge, that is necessary to apply it with advantage to the purposes of theory and practice, but also enables us to account for deviations from the general theory of its action, in a manner so consistent and reasonable, as to remove the doubts and objections of those who were inclined to dispute the whole of this doctrine.

It will also be observed in the same Treatise, that I have been able to confirm what had been formerly advanced respecting the superior force of sol-lunar influence during the periods of the *equinoxes*, by a considerable number of additional observations and arguments : an acquisition of knowledge that likewise contributes materially to a more perfect theory and practice in fevers.

For these reasons the perusal of the VIth Treatise is recommended to the young assistant, previous to every other object, as the only means by which he can possibly acquire a just and enlarged idea of the nature and forms of the class of fevers.

I meant also to have completed my plan by a full and separate investigation of all the other articles that I have mentioned, in a similar manner. But this task I am not able to perform in the extensive way I intended, and must therefore refer to the explanation I first gave of these different objects in my II^d Treatise ; and content myself, at present, with such observations respecting each as may convey, to the inexperienced assistant surgeon, a more simple and connected idea of the system explained in that Treatise ; and enable him to employ the practical rules that are deduced from it, under Problem III, as safe and immediate guides of his conduct in the treatment of fevers, on his first arrival in India.

II. OF THE NATURE OF THE FEBRILE STATE.

Respecting *the nature of the febrile state*, in my first attempts to explain it in my II^d Treatise, I was led to suppose the introduction of contagious miasmata into the intestinal canal along with the saliva. There they were conceived to act in

corrupting the mucus and other contents of the bowels ; and, that, being ultimately taken up into the circulation by the absorbents and lacteals, they might possess the power to induce the general spasmodic affection of the vascular system, with the other symptoms of the febrile state.

But whatever degree of truth may be connected with these ideas, as they are not immediately essential or necessary to the application of the practical rules that are presented in my *Second Treatise* for the management of fevers, I am anxious that the young assistant surgeon should, on the first commencement of his practice in India, defer the investigation of these theoretical questions ; and fix his mind intently on the following facts relating to the state of the intestinal canal.

1. The peculiar foetor and altered colour of the intestinal evacuations.

2. The fixed and uncommon constriction of the intestinal canal, and retention of the faeces, constantly liable to increase with the other febrile symptoms during the periods of the diurnal and nocturnal paroxysms, especially in the springs ; and as constantly disposed to relax during the periods of the morning and evening intermeridional remissions ; particularly those that happen in the morning.

3. The immediate relief and permanent advantages obtained from early, complete, and continued intestinal evacuations.

4. The great distress and danger that invariably arise from neglecting to obtain early, complete, and continued evacuations from the intestines.

An attention to these facts is peculiarly and indispensibly required in the cure of *dysentery*; which certainly differs nothing from the prevailing fever of the season, excepting in the removal or abrasion of the mucus of the intestines at some particular spots; which are thus left bare and exposed to the irritation of the corrupted contents; and ultimately to spasmodic constriction and inflammation. The removal or abrasion of the mucus may take place either from the mucus itself becoming, from a tendency to putrefaction, less firm than usual; or from the constriction or contractions of the intestine being more violent: or possibly these two causes may concur at the same time. And may not the diseased secretions of the liver, which are now allowed to exist in almost every case of real dysentery, have the power of dissolving the mucus of the intestines, or of destroying its tenacity, so as to occasion the abrasions that take place in this disease? But, whatever may be the cause, excepting this local abrasion of the mucus and its consequences, the disease is in every other respect the fever of the season: never exists without regular periodical paroxysms of the febrile state in the beginning: and excepting a more continued and unremitting attention to the intestinal discharges, and some subsidiary and palliative medicines suggested occasionally by the state of the local affection, requires no deviation in the method of cure from that recommended in other fevers of this kind. But of all the *subsidiary* means that are employed to remove the obstinate and painful spasmodic affection of the intestines, I must now observe, tho' out of place, that there seems to be none so well adapted to the nature of the complaint, and none so well recommended by the concurring testimony of many excellent practitioners,

as the occasional use of a gentle emetic in the course of the disease; and I perfectly agree, that opiates are not to be used in the earlier stage of dysentery, unless when they can be administered without interrupting or retarding the necessary evacuations, which always produce more safe and substantial relief. For this reason, although I have united opium with the evening dose of calomel, in the mode prescribed in my II^d Treatise, for curing dysenteries, I think it may, in many cases, be better to omit it; that the evacuations may be speedily and completely obtained, which never fail to give ease. Concerning the use of mercury given with another view, in the cure of dysentery and other fevers, we shall speak presently under its proper head.

III. RESPECTING THE EFFECTS OF PURGATIVES AND PERUVIAN BARK.

1. With regard to *Purgatives*. The choice and management of purgative medicines to be employed for the purpose of procuring the evacuations that are required in fevers, are sufficiently explained in the practical rules of my II^d Treatise. I have, therefore, no alteration or addition to make under this head.

It is of consequence, however, to state in this place, that the great advantage to be derived from a vigorous and systematic use of purgatives in fevers, is strongly confirmed, since these rules were first published, by a great number of respectable authorities in medicine, in every quarter of the globe.

It is further necessary to observe upon this article, that I recommend the vigorous and systematic use of purgatives des-

cribed in the practical rules of my II^d Treatise, on an accurate knowledge of the state of the bowels in these disorders ; obtained, not from the perusal of medical books, or from mere conjecture, but from an assiduous and conscientious examination of appearances in the closets of the sick, during a long succession of years. It is by this alone, that the practitioner can possibly acquire a knowledge of those particular purgatives, that are most efficacious and proper ; or learn to prescribe them with judgment and success :—and he that will not submit to these loathsome and dangerous investigations, must blunder in the dark, and fail essentially in the performance of his duty.

2. With regard to the effects of the *Peruvian bark*, in stopping, or at least, *suppressing*, the violent and dangerous paroxysms of fever, and supporting the patient against any sudden failure of strength, when administered in the steady and systematic manner recommended in the practical rules of my II^d Treatise ; and until a favourable change or crisis shall have taken place in the constitution, from a concurrence of the causes that are explained in the first part of that Treatise, and in the Treatise that concludes this collection, it is a point so fully established by the uniform result of my own experience, and by the testimony of so many respectable authorities, that it will require a very considerable weight of negative evidence to do it away.

And here I think it necessary to take this opportunity of endeavouring to account for the great diversity of opinions, that have divided physicians concerning the real character of this medicine. And when I reflect on the numerous and un-

questionable authorities, who, from their own personal experience, have testified its incomparable effects in suppressing the paroxysms of fevers in every quarter of the world, I can discover one reason only, that can be assigned for the opinion of those, who have attempted to throw it into discredit, viz. “ That they never administered it in such a manner, as to give “ it a fair opportunity of displaying its real powers :” nor have I been able to find, upon the records of medicine, one well-authenticated or unquestionable proof, of its having ever failed to produce the beneficial effects that I have ascribed to it; where it was given maturely and in sufficient quantity, and properly followed up for a length of time.

To administer the bark in such a manner as to obtain its proper effect, the following rules and precautions are indispensably necessary. 1st, To premise early and free evacuations by calomel, &c. 2d, To confide only in the substance or powder. 3d, To employ no attendant to perform this duty in whom we cannot repose the utmost confidence. 4th, To prepare the stomach to receive the first powder by a sufficient dose of laudanum given an hour before it be offered to the patient; and to secure it by proportionable doses, repeated afterwards at proper intervals, so as to keep the stomach quiet and easy; and to use the same means of preventing the medicine from passing off by stool. 5th, To give it in plenty of good wine, of which claret is the best. 6th, To be cautious not to disturb the stomach by giving nourishment or drink at improper times, or in too great quantities. And 7th, To begin early, and to continue and follow up this practice with perseverance and confidence, until the danger is over: upon which injunction I will express myself in the terms of my II^d Treatise,

— “for though it may fail to stop the meridional paroxysms during the first lunar period, even when given in large quantities for several days, yet let it not be discontinued or distrusted; for in the space of 20 years, I cannot say that I have met with any case in which I conceived it to be properly administered, where it ever failed to secure the patient in the end.” In short it is reasonable to suppose, that physicians who have most depended on the Peruvian bark, have either administered it with their own hands, or have trusted only to attendants in whom they could place the greatest confidence. On the other hand, it is natural to believe, that those who were never fortunate enough to be persuaded of its good effects, have, from this cause, been less anxious and less careful to ensure the means of giving it with strictness and attention.

IV. OF THE USE OF MERCURY AS A FEBRIFUGE.

Those were the principal objects of my attention in forming the practical rules delivered in the conclusion of my II^d Treatise, under the head of Problem III^d. But, since the publication of that Treatise, which was about 15 years ago, a new remedy has been brought into very general use in the management of fevers, viz, “*The introduction of mercury into the system so as to affect the mouth.*”

1. Considering that obstructions of the liver very frequently shew themselves in the common fevers of this country; and may with good reason be suspected to exist, in a certain degree, in all; that there is reason also to suppose, that the diseased secretions of this organ may be a principal cause of the peculiar foetid smell and altered appearance of the intestinal discharges, and may principally contribute to excite and sup-

port the febrile state ; and, above all, that the beneficial effects of the practice of giving mercury in this manner, are confirmed by extensive experience and respectable authorities in different parts of the world, we cannot hesitate to admit as an essential and valuable principle in the cure of fevers “ *The introduction of mercury into the system, so as to affect the mouth in a moderate degree, with the view of removing obstructions or other morbid affections of the liver ; of obtaining natural secretions ; and of its thus contributing, with the other means that have been described, to a speedy and permanent cure of the disease.*”

2. But with respect to “ *the idea of introducing mercury into the system in large and unlimited quantities, in cases of impending danger, from the quick reiteration of violent protracted paroxysms, with a view of stopping the fever, by inducing an immediate salivation ; and of thus rendering it a substitute, in such cases, for Peruvian bark, as a general febrifuge,*” it appears to me to be a practice that is not as yet warranted by any experience that we have obtained of its effects. I am also inclined to believe, that it has originated from fallacious appearances, which shall be explained presently, and is therefore founded on a false theory : and, no doubt, it is attended with more hazard, and much greater inconveniency and pain.

A. Referring to *experience*, the results of the trials instituted in such cases by Dr Chisholm of Grenada, by a gentleman of Jamaica as reported by Dr Jackson, by Dr Yeates and Dr Maclean in the general hospital of Calcutta, and by other medical gentlemen in Bengal since that period, do not, as far as I have been able to ascertain the events, authorise the adop-

tion of this disagreeable and doubtful means, in preference to the use of Peruvian bark : and, for the reasons that I am about to state, I suspect that the observations of Dr Maegregor, in Egypt, and of other praetitioners, are equally ineonelusive upon this point.

B. Adverting to the *theory* on which this *unlimited* use of mercury is founded, I conceive it to be *fallacious and liable to error* in the following respects :

A. In the first place, it is very well established by experience, that the most free and unlimited use of mereury, *often fails* to produce the salivation or salutary change upon which the solution of the disease is supposed to depend ; and in such cases, the unfortunate patient is left to his fate.

B. I likewise suspect strongly, that the salutary change is not only more *uncertainly*, but in general more *tediously* obtained by mercury, than by Peruvian bark ; and it must therefore be *less eligible* on this account also, in cases of imminent danger from the quick reiteration of violent and protracted paroxysms. On this question, however, I will not presume to speak decidedly ; because I do not preeisely know what has been actually done by mercury, in the way of exciting an immediate affection of the mouth. But with regard to Peruvian bark, I have seldom failed to administer, in the space of 20 hours, the quantity that was suffieient to seeure the patient a safe termination of the disease, when properly continued and followed up ; and I believe that it may generally be done in less time.

C. There is also reason to believe, that those who have adopted mercury as a substitute for Peruvian bark, in dangerous cases of fever, have been led into this new mode, *from having never seen the bark exhibited with all the expedition, energy, perseverance, and management, that are necessary to secure its beneficial effects* ; and that from this cause, they have been more readily reconciled to the unsuccessful instances of this mercurial practice.

D. It may likewise be supposed with some reason, *that an inattention to the laws of sol-lunar influence* may have contributed, on many occasions, to the rejection of the Peruvian bark, and the substitution of this mercurial plan ; and probably in two ways.

a. Regarding the *first way* in which an inattention to the laws of sol-lunar influence may occasion the rejection of the Peruvian bark, and the adoption of mercurial salivation, as a substitute for it in cases of imminent danger, it has been well established by observation, “ *that the prevailing tendency of the paroxysms of fever is to make their attacks, to increase in violence, and to continue during the period of the springs ; and that the Peruvian bark, during these periods, will often fail to produce any conspicuous change in the state of the fever ; although it will certainly secure the patient ultimately, if continued with due confidence and perseverance.*”

If, therefore, the physician should happen to be ignorant of the great confidence that ought to be placed, notwithstanding these first appearances of its inefficiency, in the vigorous and continued use of the Peruvian bark, he will naturally be led to

distrust it, when he finds that it does not exhibit some speedy and manifest proof of its salutary effects; and will be induced to substitute mercurial salivation in its stead; or any other medicine that has acquired a more recent fame: *and, in such cases, the favourable termination of the fever will naturally be referred to the last remedy; and nothing ascribed to the effect of the bark.*

b. Regarding the *second way* in which an inattention to the laws of sol-lunar influence may cause the Peruvian bark to be rejected, and mercurial salivation to be adopted as its substitute in cases of danger, I have stated in my explanation of the laws of sol-lunar influence, and I am confident that it will not escape the notice of any practitioner, who will carefully attend to the course of fevers, *that no fever ever passes the commencement of the neaps, without some conspicuous remission or perfect solution; unless when such changes happen to be counteracted by the greater parallaxes of the moon.*

At this juncture (viz. the commencement of the neaps) I have also observed, that the saliva, under a course of mercury, is naturally apt to become more free and copious; and thus will the medical practitioner, who is inattentive to these laws, be naturally led to ascribe such favourable changes in the state of fevers treated with mercury, to the sudden and copious discharge of saliva; which is nothing more than the concomitant consequence of the same cause, viz.—the diminished action of sol-lunar influence upon the human frame, producing, at the same instant, a remission in that diseased state which constitutes fever, and in that disposition of the salivary glands, which resists the secretion of saliva in a copious manner.

These deceptions, I have reason to believe, have raised the reputation of mercurial salivation far beyond its real merit. At the same time, however, although it must be obvious from these considerations, that the sudden remissions and solutions of fever in such cases, cannot be ascribed solely to the operation of mercury, I will readily admit *that this medicine must contribute greatly to render the natural changes that happen at such junctures, much more complete and permanent than they would otherwise be, by removing the morbid affections of the liver and other viscera, that support the febrile state ; probably by vitiated secretions poured into the intestinal canal.*

C. With respect to the comparative advantages of the Peruvian bark, and strong mercurial salivation, in point of *hazard, inconvenience, and pain*, we have to observe, that the uneasiness which the patient sometimes suffers from having his stomach overloaded with bark, which is the only inconveniency that attends this practice, cannot be compared to the serious evils that too frequently arise from an *excessive* use of mercury. If it fall upon the bowels, which is not uncommon, it is attended with much danger ; if in too great degree upon the mouth and throat, it produces excessive *difficulty* in swallowing, and *pain* ; and ultimately occasions a tedious confinement and cure : and a military surgeon will also reflect, that the *teeth* of a soldier are no less necessary to his vigorous and manly appearance, than to his future enjoyments and comforts in life.

Rejecting, therefore, for the various reasons that have been stated in the preceding discussion of this important question, *the practice of using mercury in the unlimited and undefined man-*

ner that we have described, as a substitute for *Peruvian bark*, in cases of impending danger, from the quick reiteration of violent and long protracted paroxysms, we are at present decidedly inclined to prefer the latter remedy. But, we readily adopt, as an essential and valuable instrument in the cure of fevers, the introduction of mercury into the system so as to affect the mouth in a moderate degree ; with the view of removing morbid affections of the liver and other viscera ; and of its contributing in this way, along with the bark and other essential instruments that we have enumerated, to a more speedy and permanent cure of the disease. With this view it will be necessary to employ it from the beginning of the fever ; but as the object of introducing it speedily would be interrupted and retarded, especially in the first stage of the cure, by the operation of the medicines that are required for obtaining regular intestinal evacuations, agreeably to the mode recommended in the practical rules prescribed in our *IId Treatise*, it will be advisable, during this stage, to introduce it principally by friction.

V. OF THE COMBINATION OF THESE MATERIALS INTO ONE SYSTEM.

Respecting the combination of the principles and means described in the preceding pages into one system, confining myself entirely to them, as objects that are *essential* and *primary*, and carefully holding out of sight those that are in general only *subsidiary* and *secondary* ; or whose nature is too imperfectly known to suggest any positive or precise rules of practice, such as contagion—general spasm—the nature of debility, direct and indirect—the effects of mercury, as a general stimulus or febrifuge, &c. I have been anxious solely to impress the plain

and intelligible idea of curing fevers by the use of purgatives and Peruvian bark, regulated by a regard to the peculiar state of the intestinal canal and the periodical changes of sol-lunar influence ; and at the same time assisted by the co-operation of mercury introduced into the system, so as to affect the mouth moderately, for the purpose of removing morbid affections of the liver and other viscera : the diseased state and vitiated secretions of which, if they do not originally produce most of the symptoms of the fever, will no doubt tend to support them, till they be restored to their natural qualities.

VI. OF THE REMEDIES EMPLOYED AS AUXILIARIES TO THIS SYSTEM.

Respecting the remedies to be employed as *auxiliaries* in the management of *this combination of essentials*, although emetics, opiates, venesection, blisters, wine, aether, and other articles, may all of them, under certain circumstances, become instruments that are essential and indispensable, yet, as they are generally used only as *subsidiary* and *secondary*, I have not thought it necessary to add any thing in this place to what has been prescribed in the practical rules of my II^d Treatise. And as those are in general sufficient to assist in securing the effects of the *principal* and *primary* means, I have abstained likewise from rendering the idea of the cure too complicated, by augmenting their number with the addition of other subsidiary instruments of great character, such as Nitric acid—fumigation—cold lavation, &c. &c. and because I really do not possess information respecting them sufficiently extensive and accurate to enable me to speak with precision.—The confounding of things that are *secondary* or *less essential*, with those

that are indispensable and primary, is the bane of every practical system, especially in medicine, and a bar to every improvement; and I have been anxious to avoid it.

VII. THE REDUCTION OF THIS SYSTEM TO PRECISE
PRACTICAL RULES.

The particular application of the whole of this doctrine, excepting only what has been added in the course of this Preface on the subject of mercury, being fully explained in the system of practical rules contained in my II^d Treatise, I will now venture to recommend these rules, improved by the addition of mercury in the manner I have proposed, *to the attention of the inexperienced assistant surgeon, as immediate and safe directors of his conduct in the treatment of fevers on his first arrival in India.*

With regard to these rules it is necessary to premise, *that the cure is supposed to commence from the first appearance or rather suspicion of the disease.* And this being a consideration, more essential perhaps than any other, not only to the object of preserving the lives of soldiers and sailors, but of securing at all times the greatest possible number fit for doing duty, it is to be hoped that it will be constantly held in view by military and naval officers, and in all medical institutions; and that patients will seldom come into an hospital in the advanced stage of a dangerous fever. When this unfortunately happens, certain modifications of these rules will of course be required, with other instruments; which, however, may be deduced from the general principles of this practice.

VIII. THE CONCLUSION.

Although these principles and instruments, and the combination of them which constitutes this system, with the practical rules that have been deduced from it, be in some respects new, I have not the vanity to suppose, that any extensive system of medicine can be formed or brought to perfection, by the ingenuity or industry of one man.—The materials of the practical system delineated in this PREFACE, are the results of the united experience and labour of many ingenious and able practitioners, continually employed in the treatment of fevers for a great length of time ; and the different successful modes of practice that now prevail in this country, admitting generally the same principles in various modifications and degrees, seem to differ from each other in this respect only ; and more or less to approximate to the system that I have explained.

In delineating THE GENERAL OUTLINE of this practical system, I therefore ascribe little more to myself than the merit of absolving a duty incumbent on my situation, by the exertions that I have made to accomplish the following objects.

1st. To secure it against the fluctuating innovations of speculative and inexperienced strangers, *by giving it the firmness and stability of a defined and intelligible form.*

2d. To prepare it for immediate use and application, as a safe conductor to the inexperienced assistant surgeon, *by reducing it to a small number of precise and distinct rules.*

3d. To present it as a sure and solid basis for progressive improvement, *by placing it on simple truths only, established by experience.*

“ Canones, sed tamen mobiles, sive axiomata inchoata, quae
 “ nobes inquiringibus, non pronunciantibus, se offerunt, pre-
 “ scribimus et constituimus. Utiles enim sunt si non prorsus
 “ veri.” *

To present moveable propositions, prepared and ready to receive progressive improvement, was the great object of the illustrious reformer of science Lord Verulam; and in this, and all my other feeble attempts to contribute to the advancement of medical knowledge in India, I have aspired to attain it in some degree.

The different Treatises that form this Collection, were published after different intervals of time in the order in which they follow each other in this Volume: and, being so many successive endeavours to place the same object in the various aspects which it progressively assumed from occasional acquisitions of new matter, they necessarily involve much repetition; and many articles that I could now wish to correct or expunge. But having no desire to conceal the mistakes into which I have fallen, nor to claim any discovery or improvement but that which belongs to me, they are now submitted to the public in their original form.

On the eve of bidding adieu to every controversy on the subject of medicine, I shall readily be excused for laying claim to the merit of being the *first* who publicly and professedly contended, that the *gonorrhoea virulenta*, is a disease totally distinct in its nature from the *lues venerea*; and does not require mercury for its cure.

* Vid. Bacon, Norm. Hist. Nat. ad Condend. Philosophiam.

This doctrine was first explained by me in a paper read before the Royal Medical Society of Edinburgh in 1766 ; and in 1767, was published as my *Thesis*, when I took my degree in medicine. Conformably to the rules of the University, I deposited a copy of it in the public Library, a copy was also deposited in the Library of the Society ; and many were distributed amongst the members and my other friends.

Dr. Trotter, the deservedly celebrated author of the work entitled *Medicina Nautica*, † being unacquainted with these circumstances, has ascribed the discovery of this doctrine to Doctor Duncan, professor of the institutes of medicine, in the University of Edinburgh. Doctor Duncan, whom I have the honour to call my particular friend, does not mention me as the author, in the work in which he elucidates this doctrine ; but the recency of my publication rendered it unnecessary ; and he certainly never intended to claim it as his own. Mr. Bell's elucidations of it are posterior to those of Dr. Duncan ; and I have the satisfaction to add, that, long before either of them were published in England, I had carried the doctrine myself to India ; and had diffused the salutary practice which it suggests pretty widely.

N. B.—Whatever Notes have been added in the course of re-printing this Collection, are distinguished from the Notes of the original Treatises, by the word *new* annexed to each.

† *Medicina Nautica*, vol. I. Appendix page 461.

TREATISE I.

ON THE
INFLUENCE OF THE MOON
IN
FEVERS.



TO THE HONOURABLE
WARREN HASTINGS, ESQ.
GOVERNOR GENERAL, &c.

SIR,

THE flattering approbation you bestowed on my manuscript, and the desire you expressed to have it published, left me no room for hesitation.

As it aspires at improvements that are important to mankind, this small specimen of my industry, had reason to hope for your attention on the grounds of its apparent utility. And if there appear in its dress, any portion of the order and becoming attire of science, I will own that I have been anxious to give it that form,

from a persuasion that it would not pass undiscerned, or unapproved.

SIR,

I have the honour to be

With great respect,

Your most obedient,

And most humble servant,

F. BALFOUR.

P R E F A C E.

THE influence of the heavenly bodies over the human frame has not escaped the notice, either of the ancients or moderns ; but their observations on this subject have not established any system or rule of certain or extensive use in the practice of medicine. The former were unenlightened by the improvements of our times, in natural philosophy and astronomy ; and the clouded and unsettled climates of the latter, seem to have denied them that constant and uniform succession of similar appearances, that is necessary to impress the idea of a general law.

THE residence of more than fourteen years in a country where, during eight months of the twelve, scarcely a drop of rain falls, or a cloud obscures the sky ; and where the influence of the moon seems to shew itself in an uncommon degree, has given me an opportunity of observing this influence in so great a number of cases, and with so little variation, that for many years past it has been

firmly established in my mind, as a fixed principle, which has directed my practice on every occasion.

IN this Treatise I have confined my observations to the following propositions concerning fevers.

I. THAT, in Bengal, fevers of every denomination are, in a remarkable manner, connected with, and affected by, the revolutions of the moon.

II. THAT, in Bengal, a constant and particular attention to the revolutions of the moon, is of the greatest importance in the cure and prevention of fevers.

III. THAT the influence of the moon in fevers prevails, in a similar manner, in every inhabited part of the globe; and consequently that a similar attention to it, is a matter of general importance in the practice of medicine.

IV. THAT the whole doctrine of the crisis of fevers may be readily explained, from

the premises established, respecting the influence of the moon, in these disorders, at the full and change.

No subject in medicine has occasioned greater division than the theory of fevers; and none, considering their frequency and danger, is of so much importance. If, therefore, in the course of these dissertations, I have hit on a line of accommodation between learned and ingenious men of different opinions; or if I have contributed to unfold a principle, by which the errors of antiquity may be rejected, and the successful practice of modern times established on a system that is consistent, and capable of demonstration, I shall consider myself singularly fortunate.

ON THE
INFLUENCE OF THE MOON

IN
F E V E R S.

PROPOSITION I.

IN BENGAL †, FEVERS OF EVERY DENOMINATION ARE,
IN A REMARKABLE MANNER, CONNECTED WITH, AND
AFFECTED BY THE REVOLUTIONS OF THE MOON.

THE bilious intermittent fever, which appears for the most part in the form of a tertian, or of a quotidian, and seldom in that of a quartan, is by far the most common fever in this country. In whatever form it presents itself, I have almost invariably observed that its first attack is on one of the three days which immediately precede the full of the moon; or on one of the three days which immediately follow it; or on one of the three days which immediately precede and follow the change of the moon. I have observed the remarkable connexion which prevails at this time evidently, at least three complete days both before and after the full and change of the moon. So that it continues at least six complete days at each. In general, I think that the days of the full and change are more powerful than any other, and those that follow the full

† Under Bengal I comprehend all the possessions of the Company in this quarter of India, with the dominions of the Vizier.

and change more powerful than those that precede ; but my observations respecting this point do not allow me to speak with any confidence. I shall, therefore, when I have occasion to mention the full and change of the moon, comprehend in my meaning the whole of the six days already described at each of these periods, without regard to any one in particular. With respect to these two periods, I cannot positively say which of them has the greatest power of producing fever.

The full and change of the moon are no less remarkable for occasioning relapses than for inducing the first attack of bilious fevers. This is a fact so well established that there are few Europeans who have resided for any time in this country, who are not sufficiently informed of it, either from their own personal experience, or from the daily proofs of it that occur in the circle of their acquaintances. But it cannot possibly escape the notice of any person who practises medicine with the smallest attention for a few months. For my own part I have observed this tendency to relapse at the full and change invariably for these fourteen years ; and in particular cases can prognosticate the return of the fever, at these periods, with almost as much confidence as I can foretel the revolution itself.

The detached examples which occur in the course of common practice, not being properly attended to, or assembled, may sometimes perhaps, prove insufficient for impressing the truth of this general observation ; but whenever a number of sick are collected together in one place, and a multiplicity of corresponding proofs are repeatedly presented at the same instant, persuasion follows insensibly, and every doubt is removed.

In the years 1773-4 I had for many months the charge of a regiment of sepoys, in the province of Cooch Behar, immediately under the vast range of mountains which separates the

northern parts of Bengal from Boutan. The disorders were chiefly fevers, or fluxes attended with fever; and in the space of the first month above 400 men were taken ill. The greatest part of these however got quit of the fever in the course of the eight days which intervened between the full and change, and, by the assistance of medicine were soon reduced to 70 or 80. But during the remaining months of our stay in that country, the sick constantly increased nearly to double this number at every full and change; falling down again as constantly to their former standard, during the eight days which intervene between these two periods. In future I shall call these intervening days the intervals.

Although I am now endeavouring to establish the superior influence of the full and change of the moon in producing bilious fevers, I must, at the same time, allow that the intervals are by no means exempted, either from first attacks, or relapses. But these happen much less frequently at this time, and when they occur, furnish arguments to support the proposition we are trying to prove: for the fits are now less severe, of shorter duration, and yield much more easily to medicine than those which happen during the full and change; and the approach of these two periods as certainly increases the violence of the disorders when present, as the coming on of the intervals, brings along with it a remarkable abatement of the symptoms; or a perfect solution of the fever.

The remittent fevers I have met with in this country have either been purely bilious, or of a putrid tendency, such as the pueea fever of Bengal, described by Doctor James Lind; or a few others of the same nature, less rapid in their progress, and resembling more the putrid fevers delineated by Sir John Pringle: to which I must add some rheumatic and nervous fevers; and likewise the fever which accompanies the eruption of the small pox.

With regard to the bilious remittents, whether you consider the particular time of their attack and relapse; the severity and duration of the paroxysm, and its disposition to remit; or the different changes that take place on their transition from the full and change to the intervals, and from the intervals to the full and change, the influence of the moon at these periods is no less remarkable in them, than it is in intermittents.

The inaugural dissertation of Doctor James Lind on the putrid fever, which first prepared my attention for this subject, furnishes many proofs of the influence of the full and change in this disorder; to which I can now add my own experience, and the consent of many other gentlemen who have been employed in practice at Calcutta.

In the few putrid fevers I have seen elsewhere, and in a small number of rheumatic and nervous fevers, the influence of the full and change never failed to shew itself in a remarkable manner; and in every case, where I have had an opportunity of making observations, I have seen it exert itself strongly in the fever which accompanies the eruption of the small pox.

But these observations are not confined to intermittent and remittent fevers. Head-achs, tooth-achs, inflammations of the eyes, asthmas, pains and swelling of the liver and spleen, fluxes, spasms and obstructions in the bowels, complaints in the urinary passages, eruptions of different kinds, and a great many more unattended by any obvious fever, assume often an intermitting form; and regularly return or increase with the full and change of the moon; and disappear or diminish during the intervals.

In general, as far as my experience extends, the attack of intermittents, during the influence of the full and change, happens at some period between eight in the morning, and six in

the evening. The accession of the paroxysm in remittents of all kinds is pretty much limited to the same time ; and the period at which their remissions seem to be most complete, is between three and eight in the morning. Fevers no doubt attack at every hour of the day and night, but this observation I think holds good in the full and change ||.

It is from the above analysis that I would venture to advance the proposition with which I set out in the beginning. I now proceed to shew that, “ IN BENGAL A PARTICULAR AND “ CONSTANT ATTENTION TO THE REVOLUTIONS OF THE MOON “ IS OF THE GREATEST IMPORTANCE IN THE CURE AND PRE- “ VENTION OF FEVERS.”

PROPOSITION II.

IN BENGAL A CONSTANT AND PARTICULAR ATTENTION TO THE REVOLUTIONS OF THE MOON IS OF THE GREATEST IMPORTANCE IN THE CURE AND PREVENTION OF FEVERS.

ALTHOUGH it be no part of my present intention to enter into a particular detail of my practice ; but solely to establish a single principle by which it is greatly directed, I find it previously necessary to deliver my sentiments with regard to the bark.

Having practised in Calcutta in 1769 and 1770, during the season in which the remittent fever of Bengal, commonly call-

|| I have no doubt of the superior influence of the moon in fevers at the time of the equinoxes, but my observations have not been accurate ; and the revolutions of the planets open a field which I have not attempted to explore.

ed the pucca fever, prevailed, I communicated my observations on this subject to Sir John Pringle, in 1772. In that treatise I related my practice at large, and drew from it the following conclusions ; which I have had no reason since to retract.

1st. That the bark * in powder is a certain cure for the putrid remittent of Bengal, commonly called the pucca fever.

2d. That there is no symptom whatever that ought to prevent it from being exhibited, after the bile is duly evacuated.

3d. That it may be given with safety at all periods of the disease ; whether in the remission or exacerbation §.

4th. That, when it is rejected by the stomach, opium will in all cases make it sit quietly ; and in sufficient quantity to stop the fever.

5th. That, in order to make a sufficient quantity sit on the stomach, or to prevent it from running off by stool, opium may be given in any moderate dose †, during any symptom, and at any stage of the disease ; whether in the remission or exacerbation.

6th. That, the true pucca fever, as far as my experience ex-

* The decoction cannot be depended on.

§ This becomes absolutely necessary, when you happen to be called too late, for after the third or fourth day, the fits are protracted so long, as to run into one another ; and when this is the case, whoever waits for a complete remission, will find himself wofully disappointed.

† That is to say, in as great a dose as is almost ever given in any case as a medicine. I have often found it necessary to give three grains in 24 hours ; and was once under the necessity of giving more than five. I begin with a moderate dose, which is repeated, or increased, just as I find occasion.

tended, was not cured by any means, except the bark, after the patient had suffered three regular returns of the fit; and that by this medicine it was easily cured, after petechiae had appeared all over the body.

I concluded my address to Sir John Pringle, with observing, that although these were the principles which conducted my practice in the cure of the pucca fever, I did not mean to advance them as rules to which there could not occur any exception; but that I never had met with any myself, and now proceeded on them with as little concern, as if none could exist.

With regard to that species of putrid fever, described by Sir John Pringle himself, and other authors of Europe, I have some reason to believe, that they might be brought to a more speedy termination, by an early and vigorous exhibition of the bark; but at all events, I have no doubt that it may be given in every stage with safety; and that it checks the fever, and prevents the putrid action from making any advances, whilst we continue at the same time to evacuate the corrupted contents of the bowels, and to supply the system with fresh stores of assuaging nourishment.

I am strongly inclined to suspect, that all the nervous fevers I have seen in this country, were nothing more than putrid fevers, in a lower degree. This suggestion, however, I leave to the examination of farther experience; and shall only observe, that in all such fevers, I have ever found the bark no less safe, than it is useful.

Very few cases of rheumatic fevers have come under my care, but in all of them the fever returned by fits; in all there was a large secretion of bile, and they were all cured by evacuations and the bark, like other bilious intermitting fevers.

Besides the pueca fever, there occur in Bengal many intermittents and remittents purely bilious, which require nothing more for their cure than early and plentiful evacuations; and in the upper and more healthy parts of the country, the pueca fever very seldom makes its appearance. Still retaining, however, the idea of this tendency in the pueca fever to run on to destruction, in spite of every evacuation, I was from this impression often misled into a premature exhibition of the bark, long after I had left Calcutta, and was removed to a climate where fevers were attended with much less danger. But when such mistakes were committed, that is, where the necessary evacuations had not been premised, they were very soon discovered and corrected. The bark was either thrown up with the bile in the course of the remission; or if it remained on the stomach, not having power to prevent the succeeding paroxysm, it seemed to render that more uneasy. These were all the bad consequences that ever ensued; and in such cases all that was required, was to repeat the common evacuations for a remission or two longer *.

Prompted by this anxiety, in the beginning of my practice, to remove the fever as expeditiously as possible, and encouraged by the safety and efficacy of the bark, in the pueca fever, I was also frequently urged to an early exhibition of it, in intermittents and remittents attended with pain and obstruction in the liver: and from observing its innocence in all such cases I have been led to a practice, which I have since found to be safe and successful; and of which I shall now communicate the substance.

1st. In intermittent fevers, which from the habit and constitution of the patient, his mode of living, his long residence

* In the hot seasons of the year, I have seldom or never had occasion to desist from giving the bark after I had once begun to exhibit it; but in the cold weather I have found it necessary to continue evacuations much longer; and if the skin can be well opened, it renders the effect of the bark much more certain.

in this country, symptoms of obstruction, the situation, or season of the year, I have reason to suspect is disposed to continue for some time, and is not likely to yield easily to evacuations alone, after the stomach and bowels are cleared of bile, (which is always produced during the fit in great quantities) it is my constant rule to give the bark as soon as possible, in general, so as to prevent the third fit: and in cases where the disorder is habitual and well known, even so as to prevent the second; provided that a sufficiently copious evacuation of bile downwards can be effected during the first fit; or early enough in the beginning of the first remission to admit of its being exhibited in sufficient quantity, to be of any use in averting or alleviating the next expected return. In cases where a tendency in the fever to continue is much apprehended, even a slight pain in the liver does not prevent me from pursuing this practice; unless I find it increase to any considerable degree by taking the bark.

I follow this practice, 1st, Because the pain and other symptoms of obstruction in the liver, which may appear, or be increased on taking the bark †, are not to be considered of any

† The large size of the liver, its warm situation in the body, and the languid circulation and peculiar nature of the blood which passes through it, suggest the probability of relaxation, accumulation, obstruction, stagnation, corruption, and irregular secretion in this organ, in warm climates. And when we consider the pains and swellings that are actually perceived in it, in the majority of Europeans who reside in this country, together with the diseased discharges of bile to which they are almost universally subject, that probability approaches in every individual nearly to a certainty. This being the case it does not appear altogether fair to load the bark with the blame of creating all the obstructions and diseases of the liver, that shew themselves on the exhibition of this medicine. The reasoning which infers that it does no more than bring to light latent diseases of the liver already existing, is supported by the prevailing probability, that the livers of Europeans residing in this country are rarely to be found in a sound state. And as the bark, when taken by people whose livers are perfectly sound, does not produce those effects, and is not accused of generating obstructions in the other glands of the body free of disease, the conclusion which is made to its discredit, has no analogy in its favour.

consequence, when compared with the superior tendency, which a continuation of the fever has to produce these effects, by accumulating the blood in the viscera, during every return of the paroxysm. 2dly, Because when any symptoms of obstruction are present, they are much more easily and effectually removed after the fever is gone; and any harm that may arise from the premature exhibition of the bark is remedied with little trouble; whereas the mistake of delaying it too long admits of no remedy whatever. 3dly, Because, independent of every consideration of danger attending each fit, it is a matter of consequence to every person to avoid the repetition of so severe an attack. 4thly, Because it is also of importance to preserve the patient's strength, in a country, where weakness always disposes to a relapse, and is attended with many other bad consequences. And 5thly, Because, by stopping the fever immediately, you secure the patient's life, a consideration superior, surely, to every other, against a great many accidents that are likely to happen at all times in the progress of this disorder; particularly from the want of proper sick-bed attendants §; which has very often proved fatal in this country.

§ For example, evacuations are absolutely necessary, after every fit, whether the bark is to be given or not. The medicine prescribed for this purpose, at a certain hour of the night, is from some cause or other neglected altogether, or not administered in a proper manner. The patient perhaps cannot be prevailed on to take it; or perhaps it is rejected immediately by the stomach, and there is no person at hand to seize the opportunity of substituting another in its place; and perhaps a false report is made to the surgeon in the morning. From causes such as these, the evacuation which should have taken place in the intermission, being neglected, the bowels and circulation get loaded with bile, the stomach becomes so weak and irritable, as to receive neither medicine nor nourishment. The fever returns with double violence, and continues without intermission; and in short, is converted into a dangerous bilious remittent. This leads me to observe that I am much inclined to suspect that the bilious fevers of the West Indies with the yellow skin and other terrible symptoms, described by authors, are owing to a neglect of plentiful and repeated evacuations downwards,

2d. All the arguments I have been advancing in favour of an early exhibition of the bark in intermittents, are equally applicable in the case of remittents; whether attended or not with symptoms of obstruction. And as these disorders are more rapid in their progress, and more dangerous, so is the necessity of this practice, in proportion more urgent. I do not recollect to have lost a single patient in this disorder, since I have proceeded on this plan; and I attribute my success, to the despatch and freedom with which I go through the first evacuations; to the particular care I always take to be certain that these have been properly effected; to my early, and, if I may be allowed the expression, even to my premature exhibition of the bark; to the perfect confidence I put in this medicine, when given in powder to a sufficient quantity; and to the free use I have made of opium in order to effect this purpose. In two or three cases of bilious remittents, where no evacuations downwards could be effected by any means, and where I began to suspect a putrid tendency, I have saved the patient's life by preparing the stomach with a large dose of opium, and throwing in, during its operation, a quantity of bark, sufficient to stop the fever.

3d. In intermittents and remittents, attended with any considerable degree of inflammation on the liver, or any other part, venesection must be instituted freely, as well as other evacuations; and in many cases blisters are necessary; after which if the fever still continue, and be not likely to stop by prosecuting this plan, the bark is to be given without hesitation: for in all the partial determinations I have met with, I have ever found the fever do much more harm in one fit, than all the bark that is necessary to stop its return.

These being my sentiments with regard to the bark, the use in the very beginning. For wherever I have met with it here, it has universally proceeded from this cause. And on the other hand I have never seen it in that form, where proper evacuations were obtained at the beginning.

and application of the facts we have established, respecting the influence of the moon in the cure of fevers, may now be explained without any interruption, and in the following manner.

1st. When an intermittent of any kind appears towards the end of the intervals, the first object to be held in view is to put a stop to it if possible, before the approach of the full and change: because, as I have already observed, the paroxysms then become more severe, of longer duration, and more difficult to cure; and will sometimes continue so long as to run into one another, and assume the form of a remittent; and afford no convenient opportunity for exhibiting the bark during the whole of that period. And although evacuations alone will generally remove the fever in the intervals, this is scarcely to be expected during the full and change.

For the same reason when intermittents appear at the beginning of the full or change, the same object must be held in view; otherwise we must not look for a solution of the fever till that period be at an end.

2d. On the other hand it is to be observed, that when intermittents make their appearance towards the end of the full and change, there is not the same occasion for a hasty exhibition of the bark: because there is a probability, if not of a spontaneous solution of the fever, at least of an abatement of its severity upon the expiration of these periods.

And we may also proceed more at leisure when intermittents make their attack at the beginning of the intervals: for we have then sufficient time before us both for plentiful evacuations, and for the bark; should it be requisite before the approach of the next apprehended revolution.

3d. One of the most important advantages to be derived

from an attention to this system, is the mode suggested by it of securing against relapses. These generally happen at the full and change, and no person who has had an intermittent, can consider himself in any degree safe at these periods, until he has perfectly recovered his strength, and removed every symptom of obstruction. It is therefore absolutely necessary to watch these returns with the greatest care; and in general the use of laxatives, and a few doses of bark given a day before, and continued every day whilst the period lasts, will prevent a relapse. When these precautions prove ineffectual, and the patient, in spite of all his endeavours, neither recovers strength, nor gets quit of the symptoms of obstructions, we are then taught to remove him with all expedition to a climate where the influence of the moon is less perceivable, and less prejudicial than it is in Bengal*.

All the common occasional causes of fever are to be avoided with more than ordinary care at the full and change; such as exposure to the sun, full meals of animal food, or whatever heats and irritates, and in short, excesses of any kind. When this system comes to be more generally understood, I flatter myself that it will suggest many useful hints for the better management of all British soldiers and sailors serving their country in warm climates; and particularly of the latter, whose diet might be regulated by an attention to these unhealthy periods, without neglecting at the same time the judicious regard that is paid at present to a proper variety and inter-change of food.

4th. With respect to bilious remittents, they are to be considered as no other than quotidian and tertian intermittents, whose fits are protracted by bile retained in the bowels, or taken up into the circulation, by obstructions of the liver and

* At Madras it is much less felt, and a removal to that settlement from Bengal is, in many cases, almost a certain cure.

spleen, by the influence of the moon, or some other cause; and in them, an attention to all the different circumstances, we have just now pointed out, is still more necessary, than in intermittents; in proportion as their progress is more rapid, their danger greater, and their management more difficult.

The tendency of such fevers to attack and remit, during the full and change, at certain times of the day, which I cannot help considering as intimately connected with the relative position of the sun and moon with respect to us at these particular periods, belongs to this place, and furnishes many useful indications in the method of cure.

I have learnt, by long experience, that all laxative and purgative medicines, as well as injections, are very uncertain in their operation, and generally disappoint, so long as any degree of fever is present. And tartar emetic itself, with all the management we are master of, will often at this time operate only on the stomach, and produce no effect whatever on the bowels. The period, therefore, at which fevers shew a tendency to remit, must be watched carefully, and purgatives must in all cases be administered on the first signs of a remission. And although these should not shew themselves distinctly, still the usual period of remission is to be preferred for this purpose. At this time they will generally operate, and evacuate the bile, which is the first, and indeed, an indispensable requisite in the cure of these fevers.

When antimonials are to be given, with a view of cutting short the fever, or of relieving the stomach of bile, the sooner they be exhibited, so much the better. But if the intention be to carry off the absorbed bile by perspiration, and to procure a more complete remission, the period we have just pointed out is to be chosen, that the operation of the medicine may concur with the tendency of the fever to remit.

This period is also the proper season for throwing in the bark, and it is often of the greatest consequence not to allow a moment of it to escape, but to begin with the earliest symptoms of its commencement ; or, when these are not manifest, at the earliest hour, at which in other cases they generally begin to make their appearance.

These hints will suffice to shew the general application of our observations respecting this period to the cure of remittent fevers. On many occasions however, circumstances are so urgent, that we are glad to seize the moment that is in our power for the exhibition of these medicines, without regard to any period whatever.

5th. Putrid, nervous and rheumatic fevers are all in this country, equally under the influence of the moon ; and in all, our attention to these observations will be of the greatest use, both in treating them, when present, and in preventing relapses.

6th. My experience in the inoculation of the small pox is confined to a small number of cases : but from the few observations I have been able to make, I am perfectly satisfied, that the full and change of the moon, interfered with the eruption, and increased the fever to a dangerous degree. I have therefore determined to avoid this accident in future, by inoculating on the 2d or 3d day of the full and change ; so that the eruptive fever may always happen in the intervals. And I have no doubt that, on farther experience this observation will become a matter of serious attention in the practice of inoculation ; and afford many useful indications in the treatment of this disorder, when caught in the natural way†.

† I have long observed, that the secretion of bile is increased at the full and change of the moon, in many cases where there is no fever. I have also observed, that whenever bile is taken up into the circulation, all wounds, sores, boils, eruptions, the gums of children teething, rheumatic pains, &c. are re-

7th. With regard to head-aches, tooth-aches, inflammations of the eyes, asthmas, pains and swellings of the liver and spleen, fluxes, spasms, obstructions in the bowels, complaints in the urinary passages, eruptions of various kinds, and a great many more which return periodically with the moon, whether attended with fever, or not, the cure entirely depends on a constant attention to these revolutions. By every succeeding return of such complaints, the parts affected grow weaker and weaker, more liable to relapse, and more difficult to cure. On the other hand, by preventing each return, the parts have a longer interval for gaining strength, become less subject to relapses, and at last recover their former tone. Therefore, when such complaints do not originate from a diseased liver, a proper attention to regimen and to the state of the bowels, a judicious derivation from the part affected, and a timely exhibition of the bark before the approach of the lunar revolution, and during their continuation, will in general succeed. But it is to be remembered, that such periodical complaints, in almost every case, are connected with a diseased liver, which is best cured by mercury §; and the bark is nevertheless to be

markedly inflamed, and irritated by it; and I have further observed, that all these complaints are remarkably inflamed and irritated at the full and change of the moon. From these premises I have been led to conclude, that the bile secreted at this time in greater quantity than usual, may be the cause of the irritation and inflammation I mention. And indeed this conclusion seems to be confirmed in a great degree by the good effects of purging in all such cases, and by the quantities of bile that are then carried off. Calomel, above all other medicines that I am acquainted with, possesses the power of carrying away the slimy and tenacious bile, with which the bowels are so apt to become loaded in this country. Query, may not the good effects of preparing for the small pox, be owing to the evacuation of all kinds of acrid bile, previously to the infection? And may not the mercury which is given on this occasion, by promoting a freer circulation in the liver, produce a more recent, and less irritating bile?

§ Long neglected obstructions of the liver, generally terminate in dropsies, which although far advanced, I have always found curable by a mercurial course, and other necessary attentions; provided that the disorder was accompanied by a tolerably smart intermittent or remittent fever. And the only cases in which

given at the full and change in such quantity as prevent relapses.

8th. As an attention to the system we have been endeavouring to explain, will teach the physician how to foresee, and provide for the various occurrences that are likely to happen in fevers; so it will enable him, not only to explain to others, in a rational manner, the past and present phenomena; but also to predict future events: a convincing proof of real science, and a certain source of reputation and confidence to him; and of inexpressible satisfaction and ease to his patient.

Having thus found, by an application of the principles established in our first proposition to the treatment of fevers, that they are not only useful in assisting us to CURE, but also to PREVENT these disorders, the truth of our second proposition follows of course.

such a course failed, were where there was no such fever to be observed. In such hydropical cases attended with a fever, the transition from the full or change to the intervals is a very critical period, and often brings on a free discharge of urine: especially if the blood be previously loaded with mercury, and a determination made exactly at this juncture, to the kidneys, by the exhibition of diuretics, such as tincture of cantharides, squills, and alkaline salts. It would be an investigation no less instructive than curious to ascertain the exact dates of all dropsies cured by sudden and unexpected discharges of urine; for I cannot help suspecting that most of these, as well as the sudden and unexpected solutions of fevers, would be found connected, in a striking manner, with the critical period I mention.

PROPOSITION III.

THE INFLUENCE OF THE MOON IN FEVERS PREVAILS IN A SIMILAR MANNER IN EVERY INHABITED QUARTER OF THE GLOBE ; AND CONSEQUENTLY A SIMILAR ATTENTION TO IT IS A MATTER OF GENERAL IMPORTANCE IN THE PRACTICE OF MEDICINE.

BEING by no means prepared to supply the comprehensive induction that is requisite to establish this proposition on an unexceptionable or certain foundation, I must therefore proceed by a method less direct ; and which although it may not afford the same degree of certainty, may however, perhaps, answer the purpose of drawing the attention of the faculty in other parts of the world, to a subject which really seems to deserve it.

By my own experience and observations, I know that the influence of the moon, at the full and change, prevails in fevers from the 13th to the 26th degree of north latitude, and we have certain accounts of it in Arabia and Persia, from the authority of the physicians of these countries. Hippocrates, who practised in Asia and in Greece, and in latitudes still higher than Arabia and Persia observed it, and wrote of it 2000 years ago. And we have testimonies of its existence in all the intermediate latitudes between Greece and Great Britain. Upon these grounds it is not extending the analogy too far to conclude, that it prevails in every inhabited northern latitude *. And these testimonies being also so many various proofs of its existence in a great number of northern longitudes, we shall

* That is to say as far north as the influence of the moon extends in the case of the tides.

likewise venture to infer, that it prevails in every inhabited northern longitude. And in short, (uniting the argument arising in favour of this conclusion from these particular instances, with that derived from the known universality of the moon's influence on the ebbing and flowing of the sea) that it prevails over the whole northern hemisphere. But having proceeded thus far, we are unavoidably led to advance still farther on this analogical ground, and to conclude, that the influence of the moon prevails equally in the southern hemisphere.

The universality of the moon's influence in fevers, all over the globe, being once admitted, it will follow by a closer analogy, that its influence is exercised in a similar manner, at the same periods, and lasts for nearly the same length of time: viz. for six days at the full and change. It will follow also, that in all parts of the globe, the knowledge of this general law may be applied to the CURE and PREVENTION of fevers, in the same manner as in India; and consequently that, AN ATTENTION TO IT MUST BE OF GENERAL IMPORTANCE IN THE PRACTICE OF MEDICINE.

PROPOSITION IV.

THE WHOLE DOCTRINE OF THE CRISIS OF FEVERS
MAY BE EASILY EXPLAINED FROM THE PREMISES
WE HAVE ESTABLISHED, RESPECTING THESE DIS-
ORDERS AT THE FULL AND CHANGE.

IF the histories and descriptions of fevers had been handed down to us by medical authors, with a minute attention paid to the date of every occurrence in the course of the disease, so as to have ascertained in every case the relative situation of

the moon, I am inclined to believe that we should have found the abatement and final solution of fevers, so much connected with the expiration of the periods of the full and change, that the truth of this proposition would have appeared at a single view, without further investigation or argument. But that indispensable requisite in every species of history has been neglected. And, on the other hand, modern practice leaving much less than formerly to nature, and putting a stop to fevers in the very beginning, or interrupting them in their natural course and termination, denies us that assistance which we might otherwise receive from daily observation. I can therefore do nothing more than invite the attention of future observers to this curious and important subject, by assuring them, that I am fully convinced myself of the truth of this conclusion from my own experience, and that in almost every case where I have had an opportunity of attending to fevers at the period when the full or change expired, and the intervals commenced, I have observed almost invariably, either some symptom of the abatement of the fever, or a perfect solution.

This observation, which was first made on common bilious and rheumatic fevers, first led me to conclude, that the transition from the full and change to the intervals, is *a favourable critical period* in fevers; and that all the days of the interval are also *favourable*. And as I have likewise been able to observe almost invariably, an increase of the fever upon its passing from the intervals into the full and change, and during the continuation of these periods, and frequently death, I therefore ventured to conclude, that the transition from the intervals to the full and change, is *an unfavourable critical period* in fevers; and that all the six days formerly described are also *unfavourable*; in other words, that “*along with the full and change of the moon, there is constantly recurring some uncommon or adventitious state or quality in the air, which increases fever, and disposes to an unfavourable termination or crisis; and that along with the intervals, there is as constantly recur-*

*“ ring a state or quality in the air, opposite to the former, which
 “ does not excite, but diminishes fever, and disposes to a favourable
 “ crisis *.”*

The histories of fevers delivered to us by Hippocrates being deficient in the essential requisite of date; and his account of crisis so much mixed with a particular theory of his own concerning the original stadia of fevers; and also with some vague and unphilosophical ideas respecting the aspects and conjunctions of benign and malevolent planets,—any attempt to reason on what he has delivered to us on this subject, would be a work of mere conjecture, and afford but little satisfaction. It is more to the purpose to say, that since I have given my attention to it, I have met with no turn or termination in bilious, rheumatic, or nervous fevers, which I have not been able

* If this be true, besides having established these propositions, we shall have also approached by a very considerable step towards a more intimate acquaintance with the more immediate cause of fever: for, by a comprehensive system of experiments and observations, we shall now be able to ascertain the nature of that state or quality in the air, which causes so essential a difference at these periods. Such a system would require a journal of every lunar day, containing a co-temporary record of fevers and other disorders; of the human body in a state of health; of the heat, moisture, and weight of the atmosphere and various winds; of experiments and observations made on the state, electricity and putrefaction; of the various revolutions of the sun, moon, and stars; and of a great many other circumstances that would be suggested occasionally, in such a manner as to ascertain their relative states and situations; and finally their connexion as causes and effects. An undertaking of this kind would be more than enough for the constant employment of one man, and is far out of my reach. But it belongs to this subject, and is worthy of being remarked, that it is a fact established in this country, on general observation, that the tendency of meat to spoil, is much greater at the full of the moon than during the intervals. I know also from experience, that all seasons of the year the full and change seldom fail to produce, for some time, an uncommon calmness, heat and closeness in the air; and if I be not mistaken, it is that calmness, heat and closeness, which lay the foundation of the winds which prevail so much about these periods.

to explain to my own satisfaction on this system ; that I have also been able to predict their turns and terminations with much certainty ; and that the duration of such fevers is not limited to any fixed *critical period* depending on odd or even days, but is constantly connected with the *favourable and unfavourable critical periods* I have just described. And whenever these *periods* interfere with the eruptive fever of the small pox, I am convinced, from experience, that they are to be considered in no other light.

From what has been already observed on the subject of the putrid or pucca fever of Bengal, I have no doubt in determining, that their *favourable and unfavourable critical periods* are the same with those of the bilious, rheumatic, nervous fevers ; and of the eruptive fever of the small pox. But to reconcile to this doctrine, the putrid fevers described by Sir John Pringle, Mr Tissot and Dr Hillary, terminating regularly in 14, 17, and 19 days, is a matter of great difficulty. And I must here once more regret, that all these histories are materially deficient in being destitute of every kind of date ; and that I am again reduced to the necessity of advancing with the faint and partial light of my own experience.

In the course of my endeavours to account for these facts, which at first seemed to establish a limited duration to particular fevers, independent of any *favourable or unfavourable critical periods*, and therefore to militate strongly against our present theory, I was led into the following train of conjecture.

In the case of putrid fevers, continuing 19 days, I supposed that there must have been a strong putrid tendency in the habit, and that the febriferous† influence of the air, which pre-

† If it be confirmed by farther experience, that this febriferous influence is constantly connected with an increased tendency to putrefaction, which there is some reason to suppose from the fact related in the preceding note, the account

vails at the full and change, co-operating with this tendency at these periods, had the power of producing a fever on the second day from their commencement; and that before means could be used to stop, or correct this disposition in the patient's habit, the fever continued to run on through the first full or change, and succeeding interval, and also through a second full or change; but that the putrid tendency being now in some degree overcome by medicine; and at the same time the febriferous influence of the full or change removed by the arrival of the second interval, a crisis, of consequence, immediately took place at this juncture, just about 19 days from the first attack.

In the case of putrid fevers, continuing only 17 days, I supposed that in them the putrid tendency of the habit was somewhat less at the beginning than in the former case; and that the febriferous influence of the full or change, had not power to excite a fever until the fourth day of the period, when the putrid tendency was farther advanced; that the fever continued to run on during the remaining days of that full or change, through the succeeding interval, and also through another entire full or change, in the same manner as the fever of 19 days; and that at last, from the concurrence of the same causes, it terminated critically, immediately on the commencement of the second interval, just about 17 days from the first attack.

And lastly, in the case of putrid fevers continuing only 14 days, I supposed that the putrid tendency in them being still less than in those of 17 days, the febriferous influence of the full or change had not power to excite a fever, until the very close of the *period* when the putrid tendency was sufficiently advanced, or towards the beginning of the interval; during

we have given of the various duration of putrid fevers will be rendered still more probable and intelligible; and be applicable to every fever of this kind, whether longer or shorter than those which we have here specified as examples to illustrate the theory of the whole class.

which interval, and the whole of the succeeding full or change, it continued to run on, and at last, from the concurrence of the causes we have just explained, terminated critically, immediately on the commencement of the second interval; just about 14 days from the first attack.

Since I became possessed of these sentiments, regarding crisis, and the above theory of putrid fevers, I have had an opportunity of meeting with only four or five cases of this kind. In one of these the fever continued exactly seventeen days, and terminated completely and finally on the commencement of the second interval; a circumstance which afforded me no small satisfaction, as I had predicted the crisis on the theory just explained, at that very juncture, and was looking for it with anxious expectation. In the other cases I had not an opportunity of ascertaining the beginning of the fever, nor consequently the exact time of its duration; and a perfect solution did not take place, as I had expected on the approach of the second interval; but in all of them the disorder took so *favourable* a turn at this *period*, that it might be called, without impropriety, a crisis of the fever.

Whether I have hit on a just explanation of the cause of that variety which appears in the duration of these fevers, is a question which I shall leave to the decision of farther experience. But I will not scruple to pronounce, even from these few examples, that they, as well as the other fevers of which we have already spoken, have their *favourable* and *unfavourable critical periods*: and that these are no other than what we have already described, viz. the full and change of the moon.

The detail which I have brought to support my opinion, respecting *favourable* and *unfavourable critical periods* is now only defective in examples of inflammatory fevers; and although I cannot say, that I have had an opportunity in this country of making observations on any that could be esteem-

ed purely and solely inflammatory ; yet as I have seen the effects of these *periods* in partial inflammations, in fevers attended with inflammatory symptoms, and in every other kind of fever, I shall not consider them as any exception to the general rule.

Whilst we employ this doctrine of *favourable* and *unfavourable critical periods* to explain the different crisis of fevers, the ancient theory of *concoction* must of course fall to the ground.

But there is no necessity on that account for rejecting the idea of a *morbid matter*, which in many instances certainly exists ; and which we conceive may be perfectly reconciled with our present system on the following terms.

1st. That in bilious and inflammatory fevers, which we know may be stopt in the beginning, or at any other stage, a *morbid matter*, if any exist, has so small a share in determining the crisis, that it is not to be regarded in practice ; and that in all such cases the *favourable* and *unfavourable critical periods* demand our principal attention. The termination of nervous and rheumatic fevers seem also to be much more under the dominion of these *periods*, than any internal constitutional principle that I have been able to discover *, and therefore, come under the same rule.

2d. That in the small pox and measles, and other diseases, the duration of the fever seems to be chiefly determined by the peculiar nature of the infection : but that great attention is also to be paid to the *favourable* ; and especially the *unfavourable critical periods*, which may aggravate the symptoms, interrupt the natural progress of the fever, and protract it beyond its usual length.

* If it should be determined by future observation, that most of the fevers called nervous, differ from the putrid only in degree, it will probably be found also, that their progress and duration is affected by the *favourable* and *unfavourable critical periods* in a similar manner.

3d. That in putrid fevers there is a putrid tendency in the habit, to overcome which a considerable space of time is often requisite : sometimes more, sometimes less, depending on the degree to which it is advanced, and perhaps other circumstances : and that their apparent limitation, on certain occasions, to a fixed duration, arises from the influence of *the favourable and unfavourable critical periods*, exerting itself in the manner already described.

4th. That the natural tendency of the constitution, with the concurrence of other occasional causes, may produce fevers, in the intervals, independent of any assistance from the febriferous influence of the moon : and when fevers of any kind begin and terminate in the intervals, the effects of the *unfavourable critical periods* will not interfere in such cases, and must not therefore be expected.

5th. That when the internal cause of fever is very powerful, and the symptoms run extremely high, the effects of *the favourable and unfavourable critical periods* may not be observable, although their influence in such cases, nevertheless, still continues to be exerted.

To sum up the whole, it appears, that, by establishing the existence of *favourable and unfavourable critical periods* ; we have acquired the knowledge of a principle which is useful in the CURE and PREVENTION of fevers : and which also teaches to PREDICT, and EXPLAIN their various CRISIS, on grounds that are consistent and satisfactory. It is therefore unphilosophical to search for any other ; and our fourth proposition must remain unshaken until it be refuted ; not by the bare dissent of one or two, who may not have directed their attention expressly to this subject, and by whom the very phaenomena upon which the whole system is built, may have passed unnoticed or uncollected ; but by the united experience and opinions of many future accurate and intelligent observers.

TREATISE II.

ON

PUTRID INTESTINAL

REMITTING FEVERS.

*Eadem est veritatis et potestatis via et perfectio : Haec ipsa
ut FORMAE verum inveniuntur ; ex quarum notitia sequitur con-
templatio vera, et operatio libera.* BACON.

TO
WARREN HASTINGS, ESQ.
LATELY
GOVERNOR GENERAL IN INDIA,

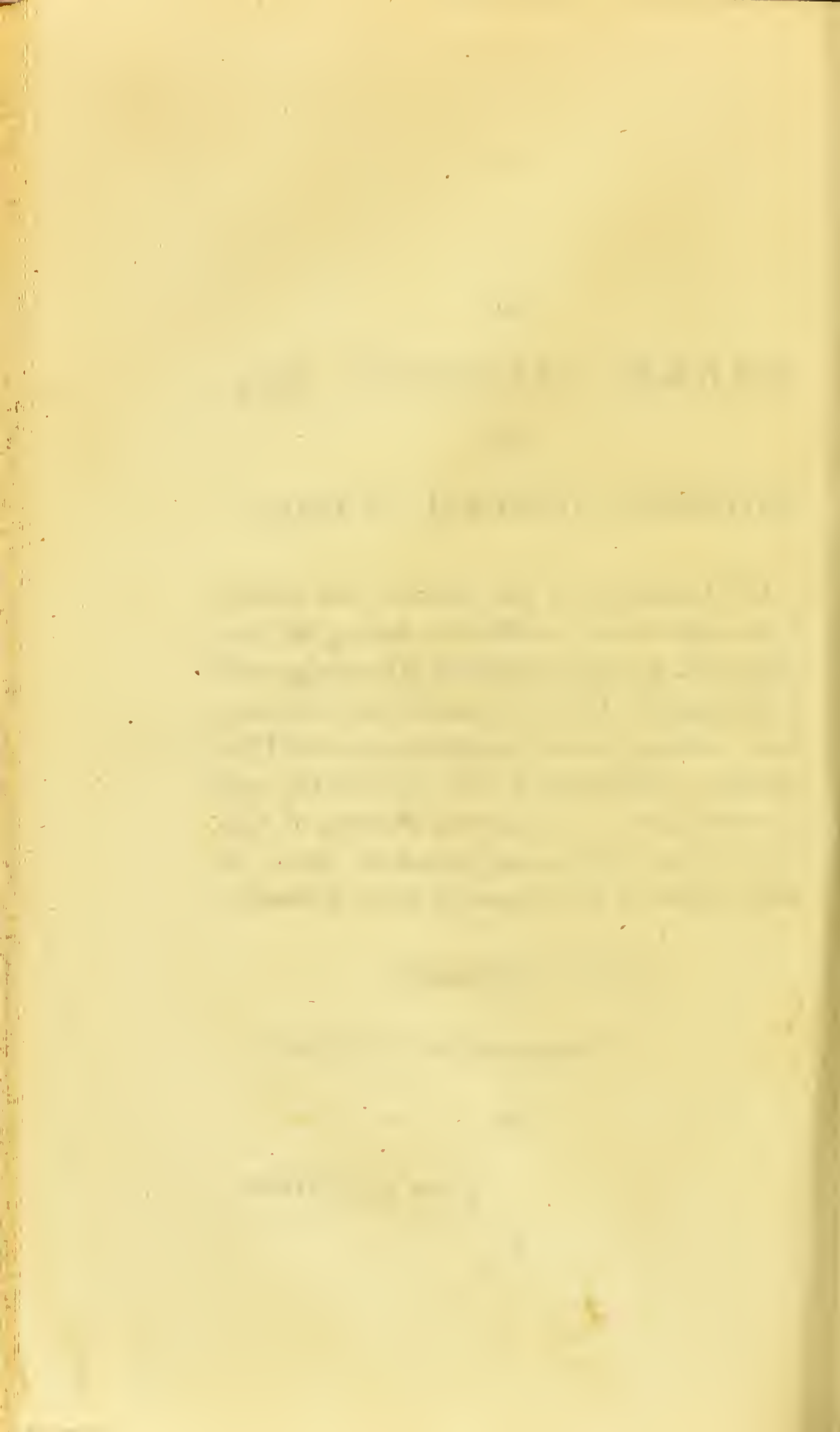
IN testimony of the zealous and liberal Patronage which he afforded, during his Government, to every species of Knowledge and Improvement that was useful and humane, THIS ENDEAVOUR to introduce into the Philosophy of Medicine a NEW PRINCIPLE, and to erect upon it an improved Method of Curing a Class of Diseases, which is, above all others, fatal to Europeans in warm Climates,

Is most Respectfully

Dedicated and Presented,

By

THE AUTHOR.



P R E F A C E.

IN a small treatise published at Calcutta in Bengal, about five years ago, I endeavoured to call the attention of the medical profession to several propositions respecting THE INFLUENCE OF THE MOON IN FEVERS; and concluded with advancing, "*That the whole doctrine of the crisis of fevers might be explained from what I had established on this subject.*"

Although this proposition arose, rather from an anticipation of the course of nature, suggested by the discovery of several striking coincidences, than from any regular process of collecting and investigating the phaenomena at large, yet, finding that it enabled me to explain with ease all the phaenomena to which I applied it, I no longer doubted of its truth; and, being eager to communicate a discovery which promised to be of great and extensive use, I was from this motive induced to publish it in the imperfect state in which it then appeared; expecting that it would instantly attract the notice of every physician, and that their united observations would soon supply materials for a complete demonstration.

Perceiving, however, upon the eve of my departure from India, that five years had elapsed without contributing any thing new to establish this discovery, and even that some opinions favourable to it had been very lately retracted by their res-

pectable author *, I instantly determined to employ the leisure of my voyage, in arranging the observations and remarks I had made myself since my first publication. I must not however, neglect to add, that the observations of Dr. Jackson, published not long ago, tend strongly to confirm this doctrine †.

I.

Having deduced the general laws of action affecting the present subject from certain phaenomena observed and selected in the course of my own experience, and having shewn how these laws may be applied to explain and predict the other phaenomena, I have thus established, agreeably to the most approved principles of philosophising, a regular and connected system of theory, which cannot give way to any hypothesis or conjecture ; but must maintain its ground, until it be shewn that the shape and course of the natural phaenomena, upon

* I mean Dr. J. Lind, to refute whose new ideas on this subject it is sufficient to say, that the whole of the doctrine delivered in this treatise, is confirmed by observations made at Benaras, and other places, not less than 300 miles distant from the reach of the tides. Vide Replies to Letter ; Treatise IV.

† Dr Jackson's first observations respecting sol-lunar influence were published in 1786, in a letter to Sir Joseph Banks, about two years after my first Treatise ; and to these I allude at present.—

His second Essay on sol-lunar influence, I mean his Treatise on the Fevers of Jamaica, was published about twelve months after this general explanation of my system ; which I am sorry he had not then seen. Vide Postscript to Treatise III of this Collection.

✎ Whilst I am preparing this work for the press, I have the satisfaction to know, that Dr Cullen is disposed to admit, in a certain degree, the operation of an influence connected with the revolutions of the sun and moon.

which I have reasoned, are merely imaginary; or that I have seen them through a false medium, and that the interpretations and inferences to which they have led me are distorted and delusive.

Sol-lunar influence §, which constitutes the most active and essential principle of this new system, by most of the authors whom I have perused on the subject of fevers, has been altogether overlooked, or disregarded; and, by the few who have observed it, and paid it attention, the laws and extent of its action being very imperfectly known, and altogether undefined, it remained till now without the support of demonstration; and stood upon no better ground than that of vague and general conjecture. If, therefore, I should assume to myself the merit of having first demonstrated the true theory of the paroxysms, forms, and crisis of these fevers, I do not conceive that I shall be guilty of any injustice to those who have gone before me.

Too partial, perhaps, to an improvement which I arrogate to myself, and therefore perhaps estimating its utility at too high a rate, I have compared its importance in medicine to that of the art of finding the longitude at sea in navigation. Possessed of this, the navigator, having an exact idea of the bearing and distance of his port, and of all the dangers and occurrences that lie between, knows, with scientific certainty, when to crowd, and when to shorten sail; and how to steer his course.

§ *The influence* which we have observed being evidently connected with the revolutions both of the sun and moon, we have therefore called it, in this *Treatise*, *sol-lunar*. Vide parag. XVIII.

Possessed of that, the physician, having an accurate idea of the different causes which determine the length or duration of his patient's fever, and which produce the different exacerbations and remissions which occur in its course, is acquainted with the leading circumstances that are required to enable him to form his practice on the principles of science; and to conduct his patient through the perils he encounters with ability and success.

Instructed and directed by the principles of this new system, I have formed the general rules which are laid down for conducting the cure of these fevers, and have thus brought to a conclusion all that I proposed to deliver in the FIRST VOLUME of this treatise.

In the beginning of June 1788, this practice was explained and recommended to the medical gentlemen who acted under me in the general hospital at Calcutta; and the success with which it was attended gave me great satisfaction. I also delivered, at the same time; to these gentlemen, a form for recording medical cases accommodated to the system we have explained; and, as it may be found convenient for future observers, I have therefore annexed it to the end of this volume.

II.

Presuming, from the known uniformity and steadiness of nature, that her laws and operations have ever been the same, we infer that the phenomena which these operations have produced, have never differed from those that are to be observed at present; and thence we conclude, that the various histories of these fevers, which are to be met with in medical books,

are nothing more than so many different accounts, more or less accurate, of the different FORMS that are produced by the action of *sol-lunar influence* upon *the febrile state*, in the manner that we have seen and described them.

This being established, we substantiate, as it were, and obtain possession, of *the constant and immutable shape and course of the NATURAL FORMS* of these fevers*, which we may employ as a fixed and infallible rule, to explain the intricacies, reconcile the contradictions, and supply the defects, that are to be found in their histories; and as a standard or criterion to estimate the merit of the different authors who have written on this subject, from the days of Hippocrates down to the present time, by shewing how nearly they have approached to them, or how far they have deviated from them in their description of nature.

Advancing on this ground, we are likewise enabled to judge and criticise the theory and practice of ancient and modern times in these fevers. And proceeding still farther, we are qualified to throw new and important light on the history and nature of other diseases; and to form a new and improved system of medicine—at least for those climates where *sol-lunar influence* is found to prevail.

The above applications of our system to these different objects, for the purposes I have stated, were reserved for a SECOND VOLUME, which I meant likewise to begin in the course of the voyage; but my time draws near to an end, and finding

* Vide Aphorism III. in the INTRODUCTION.

myself obliged to relinquish this part of my undertaking, without the probability of having it soon in my power to resume it, I have, for this reason, determined to publish this, THE FIRST VOLUME, by itself, as soon as possible after my arrival in England.

*On board of the Kent Indiaman, returning from India,
May 20, 1789.*

INTRODUCTION.

APHORISM I.

‘OMNIS philosophiae difficultas in eo versari videtur, ut
‘ a phaenomenis motuum investigemus vires naturae*’.

‘ The whole difficulty of philosophy consists in deducing
‘ the powers of nature from the phaenomena of their action.’

APH. II.

‘ Deinde ab his viribus (Aph. I.) demonstramus phaenome-
‘ na reliqua’†.

‘ From these powers (Aph. I.) we are then to explain the
‘ other phaenomena.’

APH. III.

‘ Eadem est veritatis et potestatis via et perfectio : haec ipsa
‘ ut *formae* rerum inveniantur : ex quarum notitia sequitur con-
‘ templatio vera, et operatio libera’§.

‘ The road which leads to science, leads also to art ; and it
‘ consists in discovering the real *forms* of things ; from which
‘ proceed clearness and certainty in judging, with freedom and
‘ decision in acting.’

All the phaenomena of putrid intestinal remitting fevers may be referred to the force and action of two different powers, viz. *the febrile state*, and *sol-lunar influence*. These Aphorisms, therefore, prescribed by great authorities in philosophy, suggest three different problems, which require to be solved in prosecuting this subject.

* Vide Newton, Praefatione princip.

† Vide Newton, Eodem loco.

§ Vide Bacon, aph. et consil. de auxil. ment. ad access. lum. natural.

PROBLEM I.

To investigate and define the laws of the febrile state, and of sol-lunar influence, from the phaenomena.

PROBLEM II.

To employ the laws of the febrile state, and of sol-lunar influence, to explain the nature of the various forms and crisis of putrid intestinal remitting fevers, and other phaenomena.

PROBLEM III.

To employ the knowledge we have obtained of the nature of the various forms, &c. of putrid intestinal remitting fevers, to deduce and institute an improved method of curing them.

In prosecuting the solution of these Problems, the following Aphorisms present themselves as rules to direct and regulate our conduct.

APH. IV.

‘ Frustra magnum expectatur augmentum in scientiis ex su-
 ‘ perinductione et insitione novorum super vetera, sed instau-
 ‘ ratio facienda ab imis fundamentis, nisi libeat perpetuo cir-
 ‘ cumvolvi in orbem, cum exili, et quasi contemnendo progres-
 ‘ su’‡.

‡ Vide Bacon, Nov. organ, aph. xxxi.

‘ It is in vain that we expect any considerable advancement in the sciences, from introducing and erecting the new upon the old. Improvement must begin from the lowest foundation; unless we be satisfied to continue perpetually going round in a circle, with a small and contemptible progress.’

APH. V.

‘ Canones, sed tamen mobiles, sive axiomata inchoata, quae nobis inquirentibus, non pronuntiantibus, se offerunt prescribimus et constituimus. Utiles enim sunt, si non prorsus veri ||.’

‘ But the propositions which we advance and propose are no more than moveable canons, or axioms only just begun; which present themselves to us, in the course of our inquiry, unprepared to pronounce finally. They will, however, be useful, although they may not be perfect or strictly true.’

And, to bespeak from our readers a patient and candid examination of the different propositions we have been led to advance in the course of this investigation, we shall conclude our introduction with another aphorism, laid down as a rule in philosophising by the immortal Newton.

APH. VI.

‘ In philosophia experimentalī, propositiones ex phaenomenis per inductionem collectae, non obstantibus contrariis hypothesisibus, pro veris aut accurate, aut quam proxime haberi debent, donec alia occurrerint phaenomena, per quae aut accuratiores reddantur, aut exceptionibus obnoxiae. Hoc fieri debet ne argumentum inductionis tollatur per hypotheses *.’

|| Vide Bacon, Norm. histor. natur. ad condend. philosophiam.

* ‘ Quicquid ex phaenomenis non deducitur, hypothesis vocanda est.

All opinions not deduced from the phaenomena are mere hypotheses.’

Vid. Newton Princip. pag. 530.’ Et Reg. Philosoph. iv.

‘ In experimental philosophy, propositions obtained from the
‘ phaenomena by induction, ought to be held, notwithstanding
‘ any hypothesis, as strictly, or very nearly true, until other
‘ phaenomena have occurred, by which they may be rendered
‘ either more accurate or more exceptionable.

‘ This is required, that reasoning founded on induction may
‘ not be overturned by a mere hypothesis.’

ON

PUTRID INTESTINAL

REMITTING FEVERS.

PROBLEM I.

To investigate and define the laws of the febrile state, and of sol-lunar influence, from the phaenomena.

CHAPTER I.

OF A MEDICAL DIVISION OF TIME REQUIRED FOR INVESTIGATING AND DEFINING THE LAWS OF THE FEBRILE STATE, AND OF SOL-LUNAR INFLUENCE.

I.

DIVISIONS of time have been adopted in other sciences, accommodated to their several purposes; and the following seems to be indispensibly necessary for describing the forms, and explaining the nature of these fevers: and, it is conceived, will be found well suited in other respects to the purposes of medicine.

MEDICAL PERIODS OF TIME *are,*

The daily periods, or such as occur in the space of a medical day, which is the space included between six in the morning, and the same time next day; and *are,*

1. *The diurno-meridional period*, which includes about seven hours, viz. three and a half before, and as much after twelve o'clock mid-day ;

2. *The evening inter-meridional interval*, which includes the space between the diurno-meridional, and succeeding nocturno-meridional period ;

3. *The nocturno-meridional period*, which includes about seven hours, viz. three and a half before, and as much after twelve o'clock midnight ;

4. *The morning inter-meridional interval*, which includes the space between the nocturno-meridional, and succeeding diurno-meridional period. .

The monthly periods according to this system, begin three days and a half before one change of the moon, and terminate three days and a half before the next ; and are,

1. *The novilunar period*, which includes about seven days ; viz. three and a half before, and as much after the change of the moon ;

2. *The post novilunar interval*, which includes the space between a novilunar and its succeeding plenilunar period ;

3. *The plenilunar period*, which includes about seven days ; viz. three and a half before, and as much after the full of the moon ; the novilunar and plenilunar periods, are called in general the *lunar periods*.

4. *The post plenilunar interval*, which includes the space between the plenilunar and its succeeding novilunar period. The post novilunar and post plenilunar intervals, are called in general the *lunar intervals*.

The annual periods are such as occur in the space of a medical year ; and are,

1. *The verno-equinoxial period*, which includes about seven lunar periods with their respective intervals ; viz. three and a half before, and as much after the vernal equinox ;

2. *The summer inter-equinoxial interval*, which includes the space between the verno-equinoxial and autumnno-equinoxial periods ;

3. *The autumnno-equinoxial period*, which includes about seven lunar periods with their respective intervals ; viz. three and a half before and as much after the autumnal equinox ;

4. *The winter inter-equinoxial interval*, which includes the space between the autumnno-equinoxial and the verno-equinoxial periods.

CHAPTER II.

OF THE LAWS OF THE FEBRILE STATE.

SECTION I.

Putrid intestinal remitting fevers distinguished and arranged.

II.

1. **THE** low, obscure remitting fever, answering to the description of the low nervous fever of authors; the more distinct and regular remitting fever, answering to the description of the common remitting fever of warm climates; and the more violent remitting fevers, corresponding with histories of contagious, pestilential, malignant, and putrid fevers, common to jails, hospitals, ships, camps, &c. all resemble one another in the *peculiar foetor* which distinguishes their stools, and marks the seat and nature of the disease; and likewise in all other symptoms of the febrile state, with very little difference, except in the degree of their violence. They are all infectious; they prevail under the same circumstances of heat, moisture, confinement, &c. their *laedentiae* and *prodentiae* are the same; they change reciprocally into one another; and the commencement, course, and termination of their meridional paroxysms are all similar, and connected in a similar manner with the daily, lunar and annual periods.—(Vid. Ch. III. Sect. I. II. III. IV.) I have therefore, for several years past, been induced to consider them all as putrid intestinal remitting fevers.

III.

1. From their resemblance to the fevers just described (II.) in respect to the seat and nature of the disorder, the commencement, course and termination of the meridional paroxysms, and their connection with the daily, lunar, and annual periods (Vid. Ch. III. Sect. I. II. III. IV.) and almost e-

very other circumstance; I have been led to imagine that all the disorders I have met with in India under the form of dysenteries; and likewise most of those under the form of pleurisy and peripneumony, acute rheumatisms, and of many other local affections attended with fever, were nothing more than so many different cases of putrid intestinal remitting fever, accompanied with different local affections; and to be distinguished from one another in this respect alone.

IV.

1. Agreeably to these ideas (II..III.) all putrid intestinal remitting fevers, may be arranged under the following divisions.

1. *Putrid intestinal remitting fevers, without local affection.*
2. *Putrid intestinal remitting fevers, with local affection *.*

And, except in these respects where their different local affections require remedies peculiar to their nature, I conceive that the proper method of treating all putrid intestinal remitting fevers is exactly the same.

SECTION II.

The origin and progression of the febrile state investigated.

V.

1. From paying a constant attention to the state of the

■ Although I have specified and referred to these two classes such disorders only as occur most commonly, yet I suspect that many of those fevers comprehended under the general idea of pyrexiae typhodes, whether attended with exanthemata, profluvia, or other local affections, will be found, by future observation, to belong to no other. *The puerperal fever*, and that obscure lingering disorder, so frequent amongst children, and commonly called *the worm fever*, in every case that I have seen, appeared to belong to these divisions; and were entered in the same manner as *putrid intestinal remitting fevers*.—(Vide Prob. III.)

stools in these disorders (IV.) for a great number of years, I have been led to conclude, that, in all putrid intestinal remitting fevers, the *mucus* which lines the intestines, especially the smaller, is infected, and in a putrifying state ; and that this not only produces the peculiar foetor which distinguishes the seat and nature of the disease, but, being absorbed and mixed with the blood, becomes the cause of that remarkable change in the system which constitutes the *febrile state*.

VI.

1. With regard to the introduction of the contagion which produces the disease, and its progress and operation, our sentiments are these.

That, in most cases, it is probable that the contagious matter is first conveyed into the stomach and bowels along with the saliva †.

That it seems also established by experience, that the contagious matter, after being received into the stomach and bowels, may again be discharged, after remaining a considerable interval, without producing the febrile state ; and thence we conclude, that the action of contagious matter in the stomach and bowels is not sufficient to produce the febrile state, unless it be permitted to remain there for a certain length of time.

And further, that the contagion is, in all cases, found to proceed from some putrifying or putrid body ; and therefore may be supposed to operate by producing putrifaction, especially as the mucus of the intestines is a substance, from its ani-

† We do not deny the production of putrid fevers by infectious matter, admitted directly into the blood by the absorbents on the surface of the fauces and lungs, and in other parts of the body, without passing into the stomach and intestines ; and there is no doubt that fevers are also produced by the absorption of putrid matter, generated on many occasions in the body itself, without any infection.

mal nature, loose texture, and warm situation, in a singular manner exposed to this change; and, in every case where I have had an opportunity of observing it, in the stools, was in a putrifying state.

2. From these premises we conclude, first, that, in a certain time, the contagious matter operates, in the first instance, by infecting and corrupting the mucus of the intestines; and that, in all cases of putrid intestinal remitting fevers, the mucus of the intestines is first of all corrupted. We conclude, secondly, that the corrupted mucus being constantly applied to the mouths of the absorbents which cover the intestines, the absorption of a certain quantity of it is unavoidable; and consequently that a certain quantity is actually absorbed. Thirdly, We conclude, that the corrupted matter; thus absorbed, produces the febrile state; because we know that a fever similar to this is produced by the absorption of corrupted animal matter from other parts; because the febrile state does not take place in cases where the absorption is prevented by early evacuations; and because the febrile state, as far as I have been able to discern, is always in proportion to the presumable quantity and acrimony of the matter absorbed; that is to say, that the degree of the febrile state is constantly increased when the contents of the bowels are long retained, and are become highly offensive; and as constantly prevented from increasing, when they are maturely and completely evacuated. Fourthly, we conclude that the corrupted matter continues to support the febrile state, sometimes for a great length of time; and probably until some favourable change, (Vide XXXII. 5.) in the state of the body disposes it to be evacuated by the skin, kidneys, &c. or until it be counteracted by certain medicines ‡. And we conclude ultimately, that by preventing the absorption of the corrupted matter from the intestines, by an early and complete evacuation of their mucus and other contents at the beginning, the febrile state, and

‡ Particularly the Peruvian bark.

all its consequences, may be effectually prevented from taking place.

SECTION III.

The leading symptoms of the febrile state enumerated.

VII.

1. The febrile state of putrid intestinal remitting fevers, shews itself by a peculiar putrid foetor of the stools, in which the mucus appears in a loose, frothy, putrifying state; by an increase of the heat of the body, restlessness, and frequency of the pulse, denoting irritation; by costiveness, and other circumstances denoting a spasmodic constriction of the intestinal canal §; by appearances in the state of the urine, denoting a spasmodic affection of the kidneys; by circumstances in the state of the perspiration, denoting a spasmodic affection of the skin; by a particular state of the tongue, denoting a spasmodic affection of the vessels on its surface; by circumstances in the secretion of the bile, denoting a spasmodic affection of the secreting vessels, or of the ducts through which it passes into the intestines ||; by disturbed sleep, flushing of the face, red-

§ *The tendency to costiveness, with a preternatural constriction of the bowels, constitutes a diagnostic, so constant, striking and distinguishing, and so common to all the fevers I have ever met with, and throws so great a light upon the nature and symptoms of these disorders, and the method of curing them, that I should be greatly inclined to give it a place in the general character of the class of pyrexiae. But this is not all: From the observations I have made in India, not only upon men, but upon dogs and horses, I am much inclined to believe, that in all animals, even when in health, there prevails in the bowels, during the lunar periods, a stronger tendency to contract, and to retain their faeces, than during the interlunar intervals. And should this opinion be confirmed by future observation, considering the great proportion of blood that is given to the intestines, we conceive that it will go a great length to account for the periodical plumpness of particular animals at the lunar periods; and for many phenomena in the human economy, particularly in the constitution of females, which have never been well understood.*

|| At the very beginning of putrid intestinal remitting fevers, and also about the time of their final crisis and termination, I have often observed copious dis-

ness of the eyes, tendency to delirium and head-ach, and other symptoms, denoting a determination towards the head; and by a loss of appetite, exertion, and strength, with other symptoms, not so easily referable to any obvious or immediate cause.

SECTION IV.

The nature of the febrile state inferred.

VIII.

1. From the above history (VII.) we are led to imagine, that the febrile state consists in a *spasmodic affection of the vascular system*, excited and supported by the *morbific matter* absorbed from the intestines into the blood; and manifesting itself in a particular manner in the state of the small vessels of the tongue, skin, and kidneys.

SECTION V.

The laws of the febrile state deduced and defined.

IX.

1. Seeing that the spasmodic disposition and morbid matter may each of them exist in a great variety of modes and degrees, all capable of being combined with each other, we deduce from these premises the following axioms, with regard to the nature and power of the febrile state.

A X I O M I.

“ The possible modifications of the febrile state are exceedingly numerous and different in their nature from each

charges of recent bile; but as the fever advanced, and remained at its height, such discharges have frequently ceased to appear; and I have been led to suspect, from these circumstances, that the passage of the bile into the duodenum, during this interval, was altogether stopt; and that there was a spasm on the duct, which did not yield until the fever began to give way, or tend towards a solution.

“ other; and consequently in their power to concur with
 “ sol-lunar influence, in producing and supporting meri-
 “ dional paroxysms.”

X.

2. With respect to that power of the febrile state by which fevers are enabled to come to an end, it is fully established by observation, that fevers very seldom terminate in less than four days, and do not in general continue longer than twenty one; from which may be inferred the following axiom as a Law of its nature :

A X I O M II.

“ The disposition of the febrile state, by which fevers are
 “ brought to a crisis, and which may therefore be call-
 “ ed the critical disposition of the febrile state, very
 “ seldom requires less than four days to attain that de-
 “ gree of maturity that is necessary for this event, and,
 “ in general, does not require more than twenty one.”

2. From this axiom the following Corollaries, which we shall hereafter find of great use in explaining the phaenomena, seem to arise as a necessary consequence.

COROLLARY I.

“ The maturity of the critical disposition of the febrile state
 “ advances progressively; and the longer that the fever
 “ has continued is the better prepared to accomplish a
 “ crisis; *et vice versa*.”

COROLLARY II.

“ As the maturation for crisis goes on progressively after
 “ the fever has appeared, so there is every reason to be-
 “ lieve that a similar progressive change goes on, whilst
 “ the disease is still in its latent state; bringing to matu-
 “ rity the disposition to paroxysms.”

CHAPTER III.

OF THE LAWS OF SOL-LUNAR INFLUENCE.

SECTION I.

Of the origin of the meridional paroxysms of the febrile state.

XI.

1. IN the course of a very careful attention to this subject for many years, I have observed that, during the diurno-meridional and nocturno-meridional periods, the symptoms of the febrile state are liable to certain remarkable exacerbations, which very rarely attack at any other times; and which we have therefore denominated *diurno-meridional*, and *nocturno-meridional paroxysms*.

SECTION II.

Of the connection of the meridional paroxysms with the meridional periods and inter-meridional intervals; and their different tendencies at these times.

XII.

1. Under this head I have to observe, first, that as far as my experience extends, the nocturno-meridional paroxysms, whether they attack in the lunar periods, or interlunar intervals, generally appear before the diurno-meridional paroxysms, and may be discovered in the nocturno-meridional period disturbing the patient's rest, and forming only obscure nocturno-meridional paroxysms, frequently not at first recognised by the patient himself; or scarcely recollected after the slumbers which succeed it, in the morning inter-meridional remission: Secondly, that in the course of the disease the symptoms ge-

nerally run considerably higher in the nocturno-meridional paroxysms than in the diurno-meridional: And thirdly, that the nocturno-meridional paroxysms often continue to return in the course of the interlunar intervals after the diurno-meridional paroxysms have ceased to appear.

2. From these observations (XI. 1. and XII. 1.) we are led to this inference, ‘ That the prevailing tendency of the ‘ meridional paroxysms is to attack in the diurno-meridional ‘ and nocturno-meridional periods, and not in the inter-meridional intervals; and that it is the prevailing tendency of the ‘ nocturno-meridional paroxysms to appear sooner in the beginning of the disorder, to go to a greater height in its course, ‘ and to disappear later in the end, than the diurno-meridional ‘ paroxysms.’

XIII.

1. In the evening and morning inter-meridional intervals, the diurno-meridional and nocturno-meridional paroxysms are succeeded by their respective *inter-meridional remissions*.

2. With respect to the *evening inter-meridional remissions*, at the beginning of the disease, they are sufficiently distinct and evident; but during its progress, especially when the symptoms run high, the fever goes on, and they are generally, in such cases, so obscure as not to be observed.

3. With regard to the *morning inter-meridional remissions*, they are in all cases more distinct and obvious, than those of the evening; and I have never seen the nocturno-meridional paroxysms, howsoever high the symptoms, run on into the succeeding diurno-meridional paroxysms, without some evident abatement or remission in the course of the morning inter-meridional interval.

4. From these circumstances (1. 2. 3.) we draw this inference, ‘ That the prevailing tendency of the remissions of the

‘ meridional paroxysms is to take place at the inter-meridional intervals, and not at the meridional periods ; and that it is the tendency of the morning inter-meridional remissions to be much more complete than those of the evening.’

SECTION III.

Of the Connection of the Meridional Paroxysms with the Lunar Periods, and Interlunar Intervals ; and of their different tendencies at these times.

XIV.

1. Meridional paroxysms, whether diurnal or nocturnal, almost universally make their first appearance in one of the seven days included in the *lunar periods* *, sometimes towards the beginning, sometimes towards the middle, and sometimes towards the end.

2. During the lunar periods, the nocturno-meridional paroxysms may generally be discerned earlier, as we have already observed (XII), than the diurno-meridional. But these likewise soon appear, and become conspicuous ; and, for the most part, after a few returns, increase in their violence, and run on, without any obvious abatement, through the evening inter-meridional interval ; and, being confounded with the succeeding nocturno-meridional paroxysms, no remission is observed to take place until the morning inter-meridional interval. The diurno-meridional paroxysm running on in this manner, forms, as it were, only one continued paroxysm ; and this is the form which they generally assume in the course of the lunar periods ; especially towards the 4th, 5th, 6th, and 7th day.

3. Whilst the meridional paroxysms almost always make

* The lunar period in which the meridional paroxysms make their first appearance is denominated *the first lunar period* ; and that which succeeds is denominated *the second lunar period* ; and so on.

their appearance (1.) and increase in their violence (2.) during the lunar periods, it is no less remarkable, that they seldom, and almost never disappear or subside of their own accord, so long as the lunar periods last.

4. The meridional paroxysms, after having made their attack, and gone through the first lunar period, either disappear at once, on the commencement of *the first interlunar interval*†; or becoming much more slight and obscure, and generally nocturnal, diminish daily, and disappear gradually, in the course of the interval; or still running on in a moderate manner thro' the whole of the interval, enter into *a second lunar period*.

5. Having entered into a second lunar period, the meridional paroxysms suffer an exacerbation, nearly similar to that which they suffered in the first; and thus continue going on till the commencement of *the second interlunar interval*.

6. Upon the commencement of the second interlunar interval, the febrile state, for the most part, disappears, and the meridional paroxysms cease to return; or continue only for a few days obscurely, and subside gradually; and terminate, sooner or later, in the course of the interval.

7. Although the more common course of the meridional paroxysms is to cease in the first and second interlunar intervals, it is, however, not uncommon for them to continue thro' many more successive lunar periods, and interlunar intervals, with the alternate exacerbations and abatements that are peculiar to each stage. In cases of this kind, the symptoms of the meridional paroxysms becoming less violent, the lunar periods are consequently less strongly distinguished. And, in the interlunar intervals, it often happens, that a feverish quick-

† The interlunar interval which succeeds the first lunar period, is denominated *the first interlunar interval*; and that which succeeds the second lunar period is denominated *the second interlunar interval*; and so on.

ness in the pulse is the only evident symptom that remains of the febrile state; and shews that the disease, not being come to a final termination, lurks within, and that there is still reason to apprehend a return or relapse. But, however obscurely the symptoms may proceed, they may, in most cases, be traced by a proper attention; and I have marked their progression to a 3d, 4th, 5th, 6th, and even 7th interlunar interval.

8. From these observations respecting the connection of the meridional paroxysms with the lunar periods, we are led to the following inference: ‘ That the prevailing tendency of the meridional paroxysms is to make their attack, to increase in their violence, and to continue during the course of the lunar periods; and that, as far as we have been able to discern, there is no difference, in this respect, between the novilunar and plenilunar periods.’

XV.

1. Meridional paroxysms sometimes make their attack in the interlunar intervals ‡; but seldom in proportion to the frequency of their attacks in the lunar periods; and running on, in different cases, for a longer or shorter time, just like meridional paroxysms which attack in the lunar periods, terminate also, as these do, sooner or later, in the different interlunar intervals.

2. But it is remarkable that, when the meridional paroxysms make their first attack in the interlunar intervals, it is

‡ The interlunar interval in which the meridional paroxysms make their first attack, is denominated *the exciting interlunar interval*; and the lunar periods, and interlunar intervals subsequent to it, are distinguished as *first, second, third, &c.* in the order in which they occur, just as in the case of those making their first attack in the lunar periods:—by which means the course, duration, and forms of these fevers, whether they attack in the interlunar interval, or in the lunar period, are described exactly in the same terms, and without any confusion; as will be seen hereafter in the table of FORMS.

generally towards the end of the interval, and not more than two or three days before the beginning of the lunar period; and I have seldom observed them making their first appearance at the beginning, and almost never at the middle of the interval.

3. Examples of meridional paroxysms beginning in the interlunar intervals happening seldom in proportion to those which begin in the lunar periods, lead to a different inference with respect to their tendency; and only authorise us to say,

‘ That there is likewise a tendency in the meridional paroxysms to attack in the interlunar intervals, which, however, compared to that which prevails in the lunar periods, is weak and inconsiderable; and that their prevailing tendency at these junctures inclines them strongly to defer their attack, to abate in their violence, and finally to terminate and disappear. In the post-novilunar and post-plenilunar intervals, the tendency seems to be equal; and in both it appears to be greater towards the end and beginning than in the middle.’

SECTION IV.

Of the connection of the meridional paroxysms with the equinoctial periods and the inter-equinoctial intervals; and their different tendencies at these times.

XVI.

1. Putrid intestinal remitting fevers have their meridional paroxysms much more violent and obstinate during the equinoctial periods than the inter-equinoctial intervals; and this is far more remarkable in the autumnno-equinoctial, than in the verno-equinoctial period.

2. From what has been said (1.), it is to be inferred, ‘ That the tendency of the meridional paroxysms to be violent, and

‘ to continue, in the verno-equinoctial and autumnno-equinoctial periods, is much greater than in their respective inter-equinoctial intervals ; and especially in the autumnno-equinoctial periods.’

XVII.

1. With respect to the tendency of the inter-equinoctial intervals, my observations do not enable me to make any accurate comparison ; and I can only say, ‘ That, during both, it is inferior to that which prevails at the equinoctial periods.’

SECTION V.

The laws of sol-lunar influence deduced, and defined.

XVIII.

1. These different prevailing tendencies (XII. 2. XIII. 4. XIV. 8. XV. 3. XVI. 2. XVII. 1.) so remarkable in the history of meridional paroxysms being constantly and invariably connected with certain periods of time, during which, the sun, moon, and earth are particularly situated, with regard to each other, we are naturally led to refer them to some power or influence connected with the relative positions of these bodies ; which having assumed as an established principle, or law of nature, we denominate *sol-lunar influence* ; without pretending to say whence it proceeds, where it exists, or how it operates. We must however suggest, that, as the periodical revolutions which are observed in the state of the meridional paroxysm, coincide in a striking manner with the revolutions or paroxysms in the state of the tides, it is more than probable that they are produced and governed, by the power and energy of a common cause.

2. Estimating the power of sol-lunar influence, by the effects which it produces upon the febrile state at different periods, we are led to the following conclusions ; which we think

may be assumed for the present as so many axioms or laws respecting its *force* and *action*.

A X I O M I.

“ The force of sol-lunar influence is much greater during
“ the meridional periods than their respective inter-me-
“ ridional intervals, and seems to be somewhat greater
“ during the nocturno than their respective diurno-me-
“ ridional periods; and somewhat greater during the e-
“ vening than the morning inter-meridional intervals.”

A X I O M II.

“ The force of sol-lunar influence is much greater during
“ the lunar periods, than their respective interlunar in-
“ tervals; and during the novilunar and plenilunar peri-
“ ods it is very nearly the same; and also during their
“ respective interlunar intervals, at the beginning and
“ end of which it seems greater than in the middle.”

A X I O M III.

“ The force of sol-lunar influence is considerably greater
“ during the equinoctial periods than their respective in-
“ ter-equinoctial intervals; and somewhat greater du-
“ ring the autumnno-equinoctial than the verno-equinoe-
“ tial periods.”

PROBLEM II.

To employ the laws of the febrile state and sol-lunar influence, to explain the nature of the various forms and crises of putrid intestinal remitting fevers ; and other phaenomena.

CHAPTER I.

THE VARIOUS FORMS OF THESE FEVERS TO BE ARRANGED ACCORDING TO THEIR GENERA AND SPECIES.

XIX.

1. These laws of the febrile state and sol-lunar influence, being inferred from some of the leading and constant phaenomena of a certain portion only of the class of fevers, the object of this investigation is to ascertain how far they are true, and capable of being applied to account for the various forms, and other phaenomena of the whole class. By subjecting our theory to this trial, it may now be decided whether it ought to be received as an important and valuable discovery, or rejected at once as an idle and useless fiction. The coincidence and agreement of the phaenomena with the hypothesis is the only criterion by which it can be brought to judgment; and those who are disposed to condemn it will not forget, that the whole theory of gravitation,—the system of the Mccanique Celeste, stands upon no other ground.

2. In order to ascertain the coincidence and agreement of the forms of these fevers, by an accurate comparison with the laws that are ascribed to sol-lunar influence, and of the febrile state, it becomes necessary to exhibit them both together at

one view, in all their various shapes. And for the purpose of instituting this comparative trial, I shall, in the first place, prepare a methodical arrangement of the different species of these forms, distinguished by the *type* and *duration* of their meridional paroxysms—circumstances which being fastened to their nature by an eternal law, and inseparable, afford a character, not arbitrary *nominal* and variable; but one that is *essential* and fixed, and never to be changed; and which alone is applicable to all the purposes of science and art.

For this object, it is previously necessary to investigate the nature of *types* and durations.

SECTION I.

A preparatory explanation of the nature and origin of different types.

XX.

1. Respecting *types* it is to be observed that the paroxysms of fevers do not always return constantly, with every succeeding tide or paroxysm in the force of sol-lunar influence at the meridional periods, or after intervals of twelve hours; but sometimes only every third meridional period, making intervals of 24 hours; sometimes with every fourth meridional period, making intervals of 36 hours; and sometimes at greater distances of time; but always making intervals formed by some multiple of twelve hours. These different modes, according to which the meridional paroxysms succeed to each other, are called *types*, and they are distinguished as *perfect* and *imperfect*.

2. Types with paroxysms returning regularly with every succeeding meridional period, at about an interval of 12 hours, being, as it were, exact and *complete impressions of their prototype sol-lunar influence*, I have, for this reason, distinguished them by the title of *Perfect*.

3. Types, on the other hand, in which paroxysms do not return regularly with every meridional period, but at longer intervals, being considered as *incomplete impressions of their prototype*, are denominated *Imperfect*.

SECTION II.

A preparatory Explanation of the nature and origin of the different durations or lengths of fevers.

XXI.

1. The *durations* or lengths of fevers, according to the common mode of reckoning them, seems to be measured in a loose manner by the number of days, including the day of attack and termination, without regard to hours. This mode of reckoning, as we shall show presently, has given occasion to much ambiguity and confusion in this important point of medical history. The essential question is to ascertain the exact number of paroxysms which the patient has undergone, or has still to undergo: and, for this reason, it is obvious that the proper method of measuring the length of fevers, is by the number of meridional periods.

2. On the subject of duration it is necessary to observe, that the meridional paroxysms attacking in the manner we have described (XI. XII. XIII. XIV. XV. XVI.), and continuing in different cases for a greater or smaller number of days, form different groups or successions of paroxysms, which constitute fevers of various length or *duration*, from fevers of one day to fevers of one hundred and upwards: but two or three of this length are all that I have traced with certainty.

3. As no meridional paroxysms ever pass the commencement of those interlunar intervals in which they terminate without some sudden and conspicuous abatement or remission in the degree of their violence, such as does not occur at other times, those remarkable changes are apt to be considered as *critical* and *final solutions* of these fevers: and we have every reason to believe (XIV. 4. 6. XV.) that they have in most

cases suggested and fixed the idea of their length and duration.

4. On this ground I shall consider, at present, such varieties only in the duration of fevers, as are limited and formed by an abatement of their violence on the first day of that interlunar interval in which they terminate; desiring, at the same time, that it may be well understood, that the meridional paroxysms seldom cease all at once, immediately on the commencement of the final interlunar interval; but often continue to return, as we have already observed (XIV. 4. 6. 7. XV. 1. 3.), in a more moderate degree, and at last disappear gradually and imperceptibly.

5. With respect to the duration of fevers, it is to be observed, farther, that although they are frequently seen running on for a hundred days and longer, yet it is always to be remembered, *that their general tendency is not to exceed* the beginning of the second interlunar interval; and for this reason, I have not included, in the two first genera of this arrangement, any species exceeding in duration twenty one days; and because forms going beyond this are seldom very distinctly impressed, either by the lunar or meridional periods; and are therefore destitute of those distinguishing periodical inequalities, that are required for deciding the question of their coincidence and agreement with sol-lunar influence.

SECTION III.

The genera and species of febrile forms arranged and exhibited in a Table.

XXII.

1. In this arrangement the character of the type and duration taken together constitute the character of the different species of forms; and these being arranged under the different lunar periods, and interlunar intervals, in which they respectively commence, as so many different genera, these genera are reduced to two different orders distinguished as *perfect* and *imperfect* in point of type.

FEBRILE FORMS UNDER THEIR PROPER ORDERS, GENERA, AND SPECIES.

A. Order First comprehends Genera of Perfect Type; which are four,

Ist GENUS, which comprehends Species formed by commencing in the Lunar Periods, and terminating on the transition to the first interlunar interval, which are seven,

1st Species	1st day	of the	1st day	of the	a Form of 7 days.
2d Species	2d day	and	1st day	first interlunar interval,	a Form of 6 days.
3d Species	3d day	terminating on the	1st day	makes	a Form of 5 days.
4th Species	4th day		1st day		a Form of 4 days.
5th Species	5th day		1st day		a Form of 3 days.
6th Species	6th day		1st day		a Form of 2 days.
7th Species	7th day		1st day		a Form of 1 day.

IId GENUS, which comprehends Species formed by commencing in the Lunar Periods, and terminating on the transition to the second interlunar interval, which are seven,

1st Species	1st day	of the	1st day	of the	a Form of 21 days.
2d Species	2d day	Lunar Period	1st day	second interlunar interval,	a Form of 20 days.
3d Species	3d day	and	1st day	makes	a Form of 19 days.
4th Species	4th day	terminating on the	1st day		a Form of 18 days.
5th Species	5th day		1st day		a Form of 17 days.
6th Species	6th day		1st day		a Form of 16 days.
7th Species	7th day		1st day		a Form of 15 days.

IIIId GENUS, which comprehends Species formed by commencing in the Interlunar Interval, and terminating on the transition to the first interlunar interval following it, and are,

1st Species, commencing on the 1st day of the Interlunar Interval, & terminating on the 1st day of the first interlunar interval following, makes a Form of 28 days, &c. &c.

IVth GENUS, which comprehends Species formed by commencing in the Interlunar Interval, and terminating on the transition to the second interlunar interval following it, and are,

1st Species, commencing on the 1st day of the Interlunar Interval, & terminating on the 1st day of the second interlunar interval following, makes a Form of 35 days, &c. &c.

B. Order Second comprehends Genera of imperfect type—Quotidian, Tertian, &c. &c.

☞ “ Each of these kinds of Imperfect Types might be supposed to comprehend four different Genera, similar to those exhibited in the Table, with corresponding Species.— But these I have not seen well exemplified, excepting in different cases of the Quotidian and Tertian, which are very common in India. It will be remembered that my object, at present, is not a complete Nosology, but a general illustration of this system, for which, I flatter myself, that this table will be amply sufficient.”

CHAPTER II.

OF THE CONSTRUCTION OF THE DIAGRAM, PREPARED FOR THE PURPOSE OF COMPARING THE DIFFERENT FORMS OF FEVER WITH THE DIFFERENT FORMS OF SOL-LUNAR INFLUENCE.

XXIII.

1. **T**O establish the system of sol-lunar influence, it being required that the various forms of this supposed *prototype* of fevers, should exactly coincide with all the various forms of impressions made upon the febrile state that are ascribed to it, I have put them together, in this diagram, in a state of juxtaposition ; so that the question may be decided at once by the cast of an eye, without difficulty or hesitation. To comprehend this diagram, the following explanations are necessary :

2. The *waving line* at the top of the diagram represents the course of the moon ; and being exactly divided according to the different lunar and meridional periods and intervals, it is also the representative of sol-lunar influence in all the different fluctuations and forms of its power : and to render these more obvious and striking, they are distinguished by a swelling in the line, in coincidence with the meridional periods ;—the junctures at which its action is nearest and greatest.

3. The *horizontal straight lines* of the diagram, with oval figures or swells corresponding to the meridional periods of the moon's course, represent different febrile states impressed with paroxysms by sol-lunar influence, in different modes of *succession* and *duration*, thus constituting different febrile forms, or species of fevers ; and the paroxysms of the lunar periods being in general stronger than those of the interlunar intervals, are therefore represented by *ovals* or *swells* of a deeper impression.

4. Although in this diagram the different forms of perfect type are represented with paroxysms returning in every meridional period during the interlunar intervals, as well as in the lunar periods, it is to be remembered that this is not strictly true; but that the fever during this stage of its course often shews itself, only by paroxysms during the night; and that they are here exhibited, in this manner, for the purpose of representing the general character and tendency of fevers of perfect type.

5. Respecting *postpositions* of the paroxysms in coincidence with the postponing fluctuations of sol-lunar power, it cannot be represented by a diagram that is not moveable. But their "*tendency to postpone the time of their attack at each succeeding return,*" is a phenomenon that must not be overlooked in the history of types.

The existence of this tendency is now assumed on the foundation of the two following facts:

First, That when fevers are properly treated, their paroxysms very frequently postpone in the course of the lunar periods.

Secondly, That when fevers are properly treated, their paroxysms invariably postpone in the course of the interlunar intervals.

This tendency in the paroxysms to postpone does not in every case shew itself steadily until the commencement of the interlunar intervals, when becoming, soon after the transition of the fever from the lunar periods, entirely nocturnal, they then, evidently and constantly postpone the time of attack, later and later every succeeding night; and shew themselves at last, even within the limits of the morning interval; at which time it is very uncommon for paroxysms to come on during the lunar periods. And as the paroxysms of fevers, by postponing a certain portion of time every night, will often appear

at length at an early hour of the morning, we must be cautious not to mistake this transition from night to morning, as an anticipation; which in reality is effected by a number of different postpositions; and which, occurring in the course of the night, are apt to escape the notice of a person who does not prosecute his observations with a continued attention to this progressive change.

6. Neither can the *anticipation* of paroxysms be represented in this diagram. But this is a matter of less consequence, as it is a phaenomenon that does not occur naturally; but in consequence of improper management, or some other occasional cause.

SECTION I.

The different species of febrile forms under the 1st genus of the arrangement, compared with the different corresponding forms of sol-lunar influence to which they are ascribed.

XXIV.

1. To save the trouble of the detailed comparison that is here proposed, I have constructed the following verbal *Formula*, which being applied in the comparison of the 1st species of the 1st genus, may be employed in detail with all the rest, in the same manner; holding the diagram before us, and reading the formula as we go along.

FORMULA.

“ Here is a form of the febrile state, which commencing on
 “ the 1st day of the lunar period with paroxysms of a *perfect type*, returning successively during all the days of
 “ the period, and ceasing on the commencement of the
 “ 1st interlunar interval, constitutes thus a perfect type of
 “ 7 days: coinciding with a form of sol-lunar influence,
 “ calculated by the various corresponding fluctuations of

“ its force to excite it—to support it during its course—
 “ and to concur at last, after 7 days, in its termination.”

SECTION II.

The different species of febrile forms under the 2d genus, compared with the different forms of sol-lunar influence to which they are ascribed.

XXV.

1. To save trouble I have likewise constructed the following Formula, for the comparison required in this case, which may likewise be employed to explain all the rest of the species under this genus.

FORMULA.

“ This is a form of the febrile state which commencing on
 “ the 1st day of the lunar period with paroxysms of a perfect type, returns successively for 7 days with increasing force ; then subsiding and going on, with paroxysms much less strongly and distinctly marked, runs on for seven days more, and rises on the commencement of the 2d lunar period ; and finally continuing with paroxysms more strong and distinct than in the preceding intervals for seven days longer, ceases at last on the commencement of the 2d interlunar interval ; and thus constitutes a form of 21 days : all the while, proceeding in coincidence with a form of sol-lunar influence, calculated by the various corresponding fluctuations of its force to excite it—to support it during its course,—and to concur at last in its termination at this particular juncture.”
 And so on with all the rest.

SECTION III.

Conclusion.

XXVI.

1. I have gone through the task of this tedious and formal investigation, not with the idea that it is to produce conviction.

Or. A

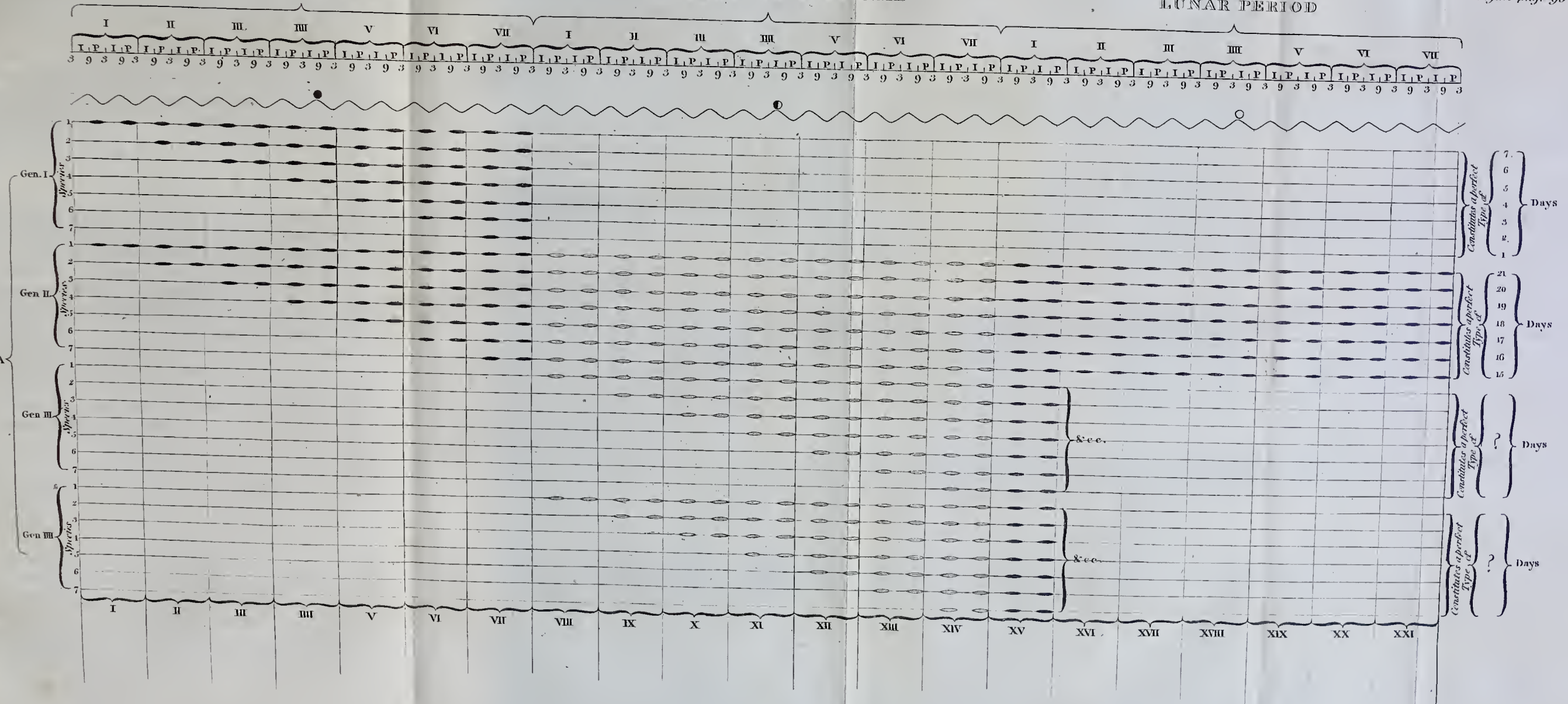
PLATE I.

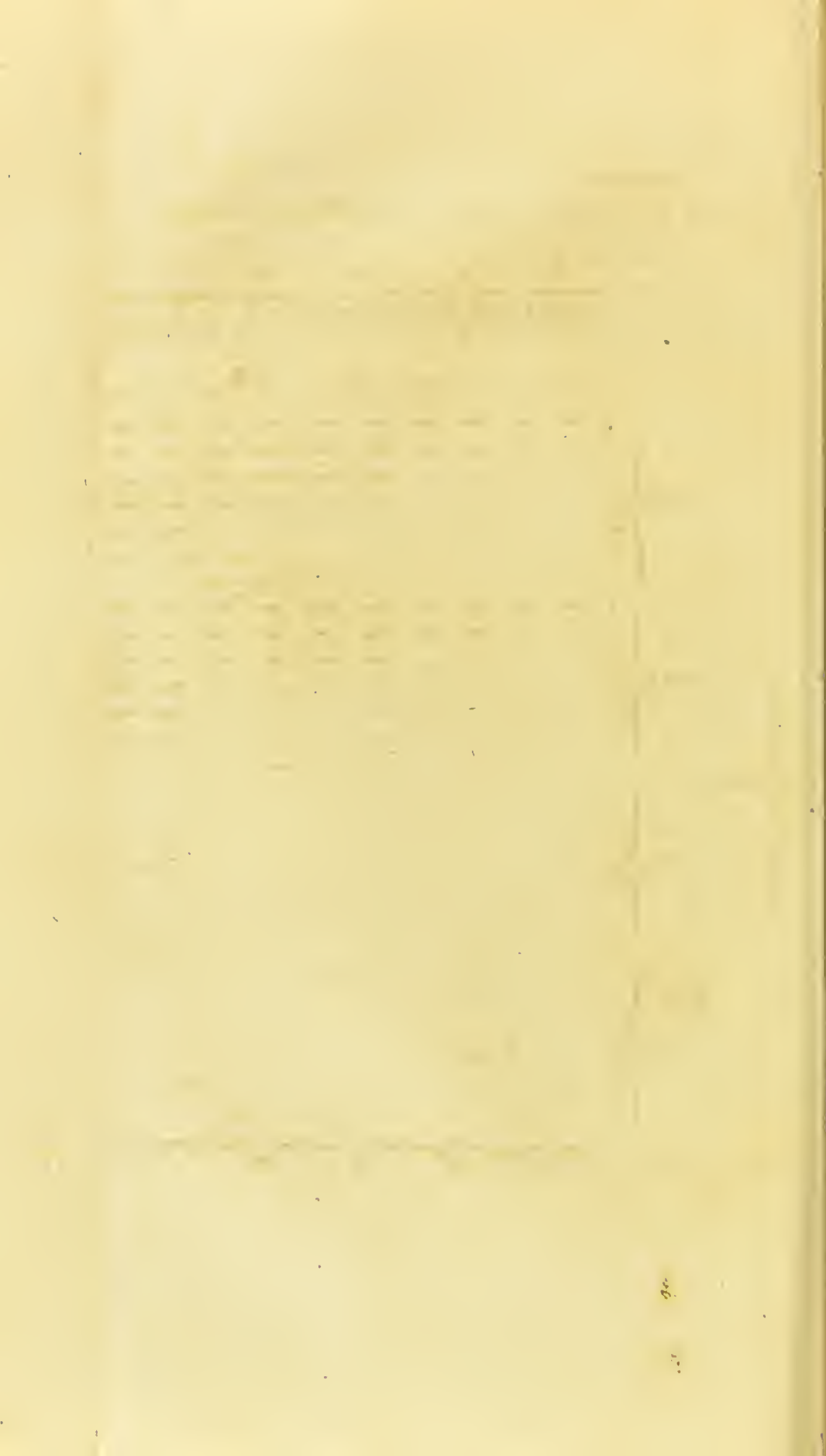
LUNAR PERIOD

INTERLUNAR INTERVAL

LUNAR PERIOD

Ref. page 96.





tion in any mind capable of advancing thus far without being convinced, but merely to shew the immoveable basis on which the theory stands, in consequence of the infinite number of coincidences on which it is supported.

Without proceeding, therefore, beyond the species of the two first genera, which are objects that have been constantly passing before my eyes for 30 years of my life, I shall now think myself warranted to express the result of my investigation in the following words.

THEOREM.

“ The fluctuating force of sol-lunar influence coinciding and
 “ co-operating in all its various stages and degrees, with
 “ the various modifications of the paroxysmal disposition,
 “ excites febrile paroxysms to attack in all the days of the
 “ springs and neaps, and supports and reiterates them ac-
 “ cording to various types until the commencement of dif-
 “ ferent neaps ; at which junctures the maturity of the cri-
 “ tical disposition happening to concur with the periodi-
 “ cal decline of sol-lunar influence, these paroxysms then
 “ subside and come to a solution or crisis ; and thus form
 “ different successions of paroxysms constituting fevers of
 “ various length or duration.”

SECTION IV.

*Of certain ambiguities that arise in reckoning the durations
 of fevers.*

XXVII.

1. Although there can be no doubt that the forms delineated in this diagram are the true outlines of the natural and immutable forms of fevers, it must, however, be acknowledged, that there may occasionally occur some little difficulty in ascertaining their durations minutely in some particular cases ; and such difficulties may occur in three different ways.

First, From the obscurity and slowness with which the fever sometimes makes its approaches ; and in such way that it might be considered, by different physicians, to have commenced a day sooner or a day later.

Secondly, On the other hand, the same kind of doubt arises, sometimes, respecting the precise time of its termination, from the obscurity of the symptoms attending the crisis.

Thirdly, Both of these causes of difficulty may happen to occur in the same case.

XXVIII.

1. Under these circumstances it is reasonable to suppose, that fevers have been referred by physicians, sometimes to a form of longer, and sometimes to a form of shorter duration than they strictly ought. A real form of 7 days, for example, may have been often put down occasionally as a form of 6 days ; and on the other hand, as a form of 8 days ; and even perhaps of 9. And there can be little doubt that the same ambiguities have occurred in estimating the durations of all the other forms of fevers.

XXIX.

1. To secure us against these ambiguities and mistakes, by ascertaining decidedly how far the fever has actually advanced in a paroxysmal state, a reference must be made to the state of the symptoms during the *nocturno-meridional period* : for it is here that it shews itself first, (at least in all perfect types,) and with least disguise.

XXX.

1. On the other hand, to ascertain the real progress of the critical disposition, we must watch with great attention, the whole course of the *first morning inter-meridional interval*, after the expiration of the lunar period ; examining with particular care, the appearances of the urine.

SECTION V.

Of the crisis of fevers.

XXXI.

1. Having thus traced the various forms of fevers from their commencement to their termination in *crisis*, we shall now conclude this part of their history, with some explanations respecting the revolution that takes place in the constitution at this important event.

XXXII.

1. In paragraph (XXII. 3.) we have observed that, “as no meridional paroxysms ever pass the commencement of those interlunar intervals in which they terminate, *without some sudden and conspicuous abatement or remission in the degree of their violence, such as does not occur at other times*, those remarkable changes are apt to be considered as *critical* and *final solutions* of these fevers; and we have every reason to believe that they have in most cases, both in ancient and modern times, suggested and fixed the idea of their length and duration.” In these words I have described generally what I shall in future call the crisis; and shall now attempt a more precise delineation.

DEFINITION OF CRISES.

“The crises of these fevers are favourable changes, which
 “never fail to take place, in some degree or other, at the
 “time of their *transition* from the lunar period to the in-
 “terlunar interval, and generally on the first morning in-
 “ter-meridional interval after it; at which juncture the
 “maturity of the critical disposition concurs with the pe-
 “riodical decline of sol-lunar influence in bringing them
 “about: and they are distinguished by one or more of
 “the following symptoms *viz.* a sediment or particular tur-
 “bid appearance in the urine; a more free and natural
 “perspiration; spontaneous stools; a cleaner, moister,

“ and softer tongue, with a more free and natural discharge
 “ of saliva ; a more loose and copious expectoration, and
 “ a free discharge of bile which seems to disappear, and
 “ to be suppressed in the course of the fever, &c. &c.”

2. The most remarkable and important of all these symptoms are the appearances of the urine, to explain which I dedicated the whole of my III^d Treatise ; and concerning which I do not hesitate to affirm, after many years of anxious and attentive observation, that there is to be observed in the fevers of India, by any person who will take the trouble to examine it carefully, a regular and constant fluctuation ; that is to say, regular diurnal and septenary changes in its character, coincident and correspondent with the periodical intensions and remissions of sol-lunar power. And as the doctrine of sol-lunar influence explains the periodical changes of the urine in a consistent and satisfactory manner, so do the periodical changes of the urine exhibit, in support of sol-lunar influence, a chain of coincidence so clear and connected, that it might be assumed on this foundation alone, without any collateral proof from the other symptoms, or the other parts of nature.

3. As these fevers never fail to shew some conspicuous abatement in the degree of their violence, not only at the commencement of the interlunar intervals in which they terminate, but likewise at the commencement of every interlunar interval through which they pass previously to the final one, these remarkable abatements or remissions, shall be called in future *intermediate or imperfect crises* * ; which will be distinguished according to the order in which they occur as *first, second, third, and fourth, &c.* and to distinguish from these the last crisis, or that with which the disease terminates, it will be

* It is more than probable that the *crises* which Hippocrates describes by the words ‘ *imperfecte judicabantur*, ’ were nothing more than *intermediate interlunar crises* ; and that those to which he applies the terms *perfecte judicabantur*, were *final interlunar crises*.—Vide Popular. Ubique.

called a *final or perfect crisis*. And crises, both intermediate and final, will be distinguished by the terms *distinct* and *obscure*, according to the symptoms.

4. From the different remarkable changes that occur in the symptoms at the final or *perfect crisis*, there appear to take place two important events; viz. a *solution of the spasm*, and a *discharge of morbid or corrupted matter*; whether induced by infection or produced in the course of the disease; and on these grounds I am inclined to suggest the following ideas respecting the *process* by which these fevers are brought to an end at the time of the crises: viz. that it is not improbable that the spasm originally induced by the infection gives way first, and that the secretories then becoming more relaxed and open, and the secretions more copious, the corrupted particles, possibly changed in their nature, will also be discharged in greater proportion; and that from the concurrence of these two circumstances in various degrees, the fever is ultimately brought to an end, sometimes suddenly and at once, but generally by degrees.

5. By introducing sol-lunar influence as a new principle in the pathology of fevers, the two remarkable theories of *concoction* and *spasm*, which have been supported with so much zeal by their respective votaries, may now, perhaps, be both in some degree admitted; and reconciled with each other.

CHAPTER III.

THE CONTEMPLATION OF THESE FORMS NATURALLY
LEADS TO IMPROVEMENTS IN THE NOSOLOGY, PA-
THOLOGY, PROGNOSTICS AND TREATMENT OF THESE
FEVERS.

SECTION I.

Of improvements in the Nosology.

XXXIII.

1. Having established by this analysis that the various forms of these fevers, which we have thus arranged under the two first genera, are not things merely fictitious or imaginary, but the true shapes and forms of real existences in nature, I have thus attained the first great step in all philosophical investigation—the advantage of surveying and contemplating them divested of covering and disguise: and it will not now be difficult to extend it to all the other genera and species under this *division* of fevers; which, for the reasons that I am about to assign, I shall in future distinguish by the title of TYPHOUS FEVERS.

XXXIV.

1. In the beginning of our explanation of the laws of the febrile state (II.) the large division of fevers comprehended under the title of putrid intestinal remitting fevers, were all considered to be infectious, and supposed to originate from the common domestic typhus as a common source; without meaning to except the fevers generally ascribed to Marsh Miasmata, the yellow fever, or even the plague itself: they may all, notwithstanding a considerable difference in their phaenomena, be conceived to be modifications of the same disease; and the system of intestinal evacuation, as far as has been hitherto ascertained, seems to be equally successful with all. It was with the view of drawing the attention of the young prac-

itioner to this important circumstance, that I was first induced to distinguish them by the title of putrid intestinal,—as in a certain degree descriptive of their nature; and well calculated to suggest the particular mode of treatment which they required. This necessity, it is to be hoped, does not now exist; and as the title putrid intestinal remitting fevers is, in some respects, exceptionable in a nosological view, I shall now restore, to this tribe of fevers, the family name of their common parent, and distinguish it from other tribes of fevers by the title of *typhous*.

SECTION II.

Of improvements in the Pathology and Prognostics.

XXXV.

1. By possessing the advantages of thus surveying and contemplating these fevers in their proper forms, it will now appear that we have been able to discover, that, along with their specific distinctions in external form, there exist likewise, in each, essential and specific distinctions in their inward constitution, which are highly necessary for a physician to know.

XXXVI.

1. With respect to the specific constitutional distinctions of different forms, it must be remembered, in the first place, that the commencement of fevers on the 1st, 2d, 3d, 4th, 5th, 6th, and 7th days of the lunar period, making between each case a difference of 24, and perhaps 36 hours, necessarily implies very different degrees of predisposition or propensity to paroxysm, in the constitution of each individual case.

XXXVII.

1. On the other hand it is no less evident that the fevers of these individual cases, thus beginning on different days of the lunar period, and having respectively to continue exposed to the action of fever, for 7, 6, 5, 4, 3, 2, or 1 days, before

their transition into the 1st interlunar interval, must each of them reach that important juncture after very different degrees of suffering ; and consequently, in very different degrees of maturity or aptitude for a critical change. (Vide par. XI. Ax. II. Cor. I.)

2. It is obvious that fevers beginning on the 1st day of the lunar period, having 7 days to complete the process of maturation for crisis before the transition into the interlunar interval ; and, therefore one day more than those beginning on the 2d day of the period, must be better prepared than them for that event, and accomplish it more completely and more frequently at this juncture ; ‘and, consequently, that fevers of ‘7 days duration must not only be more perfect in their crises ‘than fevers of 6 days, but must occur more frequently in the ‘course of nature.’

3. This rule is equally applicable in regular gradation downwards, to all the different species of forms attacking on different days of the lunar periods, and liable to terminate on the transition into the 1st interlunar interval ; forming thus respectively fevers of 7, 6, 5, 4, 3, 2, and 1 days. And what will not fail to strike with wonder, those physicians who are disposed to confine these new doctrines to the provinces of India, accords, in every point, with the history of similar fevers in Europe, from Hippocrates down to the present day,— ‘in regard to the difference observed in forms of different durations, as to the perfection of their crises, and the frequency ‘of their occurrence.’ Fevers, for example, terminating in 7 days are universally observed to be more perfect in their crises, than fevers terminating in 5 days, and far more frequent in their occurrence ; and so of all the rest from fevers of 7 days, to fevers of no more than 1 ; and as the superior frequency of fevers terminating in 7 days above all other is rationally explained in this manner, the more frequent occurrence of these terminating in 14 and 21 days is to be accounted for in a similar way.

4. The application of this principle is, in short, universal and satisfactory ; and there is no phaenomenon respecting the types, durations, circuits, relapses, and crises of fever, described by the divine Hippocrates, that are not explained in a consistent and rational manner, on this and the other principles that have been unfolded in this system.

From the internal evidence contained in the works of Hippocrates himself, I can demonstrate that he had no knowledge of this theory. But this renders his merit as a faithful and accurate painter of nature still more admirable ; and, seeing that, in the present state of medicine, he cannot easily be imitated, altogether inestimable.—Perhaps, in a distant century, and not sooner, some delighted admirer of his transcendent skill, passing as it were, along the gallery of his ancient portraits, and catching with ecstasy, by the aid of this small taper, some hidden excellence, will remember, with a feeling of gratitude, the humble artist that put it into his hand !

XXXVIII.

1. Having shewn, in the preceding paragraphs, that the circumstance of beginning on different days of the lunar period, by producing *different degrees of maturity in the disposition to crisis*, previous to the transition into the 1st interlunar interval, gives occasion to essential differences amongst these fevers *in the perfection of the crises, and in the frequency of their occurrence*, we are naturally led to inquire from what causes fevers are determined to attack on the different days of the lunar periods. But in prosecuting this inquiry I find it necessary to attempt a solution of the following questions.

1st. At what distance may the infection be received ?

2d. Is it liable to be received at any particular periods of time more than others ?

3d. What length of time is required, after it has been received into the body, for inducing *that change which constitutes the maturity of the disposition to paroxysm?*

2. With regard to the first of these questions, viz. "at what distance may the infection be received?" Although this is a question of great importance, and merits a very particular discussion, I must at present be satisfied with observing, that it seems to be well ascertained by the ingenious investigation of several eminent physicians, "that typhous infection is not received at any considerable distance, and probably almost only by those who live in the same apartments with the sick, or who are employed in the capacity of nurses and attendants."

3. With regard to the second question viz. "is it liable to be received at any particular times more than others?" proceeding on what has been said respecting the distance of receiving the infection as good ground, and taking along with it the acknowledged power of sol-lunar influence to excite and multiply fevers in the lunar period; and of course to generate and accumulate the matter of infection at this time, "there can be no doubt that infection is principally received during the lunar periods; and especially towards the last days of these periods, when it is not only more generally diffused, but possibly more active and catching in its nature: whilst, at the same time, the attendants of the sick are much more exposed to it, from nearer approaches to their persons, and a more constant attention required at this dangerous and helpless stage of the disease."

4. With regard to the third question viz. "what length of time is required after the infection has been received into the body, for inducing that change which constitutes the maturity of the disposition to paroxysm?" As the maturation for crises goes on progressively after the eruption of pa-

roxysms, "so there is every reason to believe that a similar "progressive process goes on whilst the disease is still in its "latent state, bringing to maturity the disposition to paroxysm."

5. Having obtained this general proposition respecting the progressive process of the infected habit towards the paroxysmal state, it is evident, that the attack of paroxysms on different days of the lunar period must depend greatly on the time that has elapsed since the infection was received; which varying in every different case, will prepare some to attack on the 1st day, some on the 2d day, some on the 3d, and so on with all the rest. But not being possessed of the facts that are necessary for determining the exact time that is required in each particular case, it is impossible, at present, to give to this question a more precise solution. It may, however, be of some use to point it out as a matter of future investigation, intimately connected with a necessary knowledge of the nature of these fevers.

SECTION III.

Of Improvements in the Treatment.

XXXIX.

1. Agreeably to the original intention and distribution of Chapter III. d. this Section was allotted to the purpose of conveying a general idea of improvements, naturally arising from the better defined characters of these fevers, in the method of treating them. But as this, I find, is a subject that requires more room than I can afford in this place, and will be sufficiently illustrated in the solution of the Third Problem of this Treatise, I shall now defer the illustration intended here.

CHAPTER IV.

OF OTHER PHAENOMENA ; PARTICULARLY OF THE VARIATION IN THE STATE OF THESE FEVERS, AT THE EQUINOCTIAL PERIODS, AND INTER-EQUINOCTIAL INTERVALS.

XL.

1. The changes which take place in the state of these fevers, at different seasons of the year, are the only phaenomena that now remain to be explained.

2. But, as the variation which takes place in the verno-equinocetial periods is much less conspicuous, than that which appears in the autumn-equinocetial period, and is not so precisely ascertained, we shall therefore, at present, attempt to pronounce only with respect to the latter.

3. Upon this subject we have to observe, that, as far as we have been able to discern, putrid intestinal remitting fevers are much more frequent, violent, and obstinate, during the autumn-equinocetial period than the inter-equinocetial intervals which precede and follow it.

4. In explaining the cause of this inequality, we are naturally led to adopt a mode of reasoning similar to that which was employed in accounting for the superior frequency of fevers attacking in the lunar periods, compared with the frequency of those which attack in the interlunar intervals ; and we apply it in the following manner.

It being established, that these disorders are produced by infection, and consequently that they will be propagated and spread in proportion to the number of those that are infected, and the degree of infection ; and having also shewn that the

force of sol-lunar influence to excite meridional paroxysms is much stronger during the equinoctial periods than their respective inter-equinoctial intervals; and, ultimately, that the frequency, violence, and obstinacy of these fevers, are much more remarkable during the autumnno-equinoctial period than the preceding and following inter-equinoctial intervals, we think it reasonable to infer, from these premises, the following propositions respecting the cause.

THEOREM.

“ That the superior force of sol-lunar influence at the autumnno-equinoctial periods, first, by exciting the attacks of fevers during these periods in such as are already infected, then by propagating and spreading the infection from these to others, and again by exciting these to attack, and so forth, MORE powerfully and frequently than in the inter-equinoctial intervals, operates in a circle, and thus becomes the cause of the superior frequency, violence, and obstinacy of fevers in the autumnno-equinoctial periods.”

5. From the preceding theorem arise the following corollaries, which enable us to prognosticate, with considerable certainty, respecting the state and course of these fevers, when they occur at these different seasons of the year.

COROLLARY I.

“ Caeteris paribus, fevers attacking, and having to run their course in the autumnno-equinoctial period will be more severe and obstinate, than those which attack, and have to run their course, in the inter-equinoctial intervals.”

COROLLARY II.

“ Caeteris paribus, fevers passing from the summer inter-equinoctial interval into the autumnno-equinoctial period, will incline to increase and continue.”

COROLLARY III.

“Caeteris paribus, fevers passing from the autumnno-equinoctial period into the winter inter-equinoctial interval, will incline to subside and terminate.”

By a reference to my VIth Treatise published in the VIIIth volume of the Asiatic Researches in 1803, it will appear that I have acquired very important additions to my knowledge on this part of my subject, from the labours of de La Place and de La Laude.

CHAPTER V.

THE SOLUTION OF THE PROBLEM CONCLUDED.

XLI.

1. Presuming, from the established uniformity and steadiness of Nature, that her laws and operations have ever been the same that they are now, we infer that the effects and phaenomena, which these have produced, have never differed in their nature and forms from those of the present time; and thence we conclude, that *the various histories which have been delivered to the world of such fevers, are nothing more than so many different pictures, more or less accurate, of their different natural forms, produced, like those which we have seen and described, by the action of the sol-lunar influence upon the febrile state.*—It therefore follows that their nature, origin, course, duration, and termination, and, in short, their forms and crises, have always been similar to the nature, origin, course, duration, and termination, and, in short, to the forms and crises of those which we have seen and described; and consequently, that these having been already explained and accounted for in the course of this analysis, *we have thus unfolded the whole doctrine and mystery of the forms and crises of these fevers:* Which was proposed to be done.

PROBLEM III.

To employ the knowledge we have obtained of the nature of the various forms, &c. of putrid intestinal remitting fevers, to deduce and institute an improved method of curing them.

CHAPTER I.

THE GENERAL RULES DEDUCED AND PROPOSED.

XLII.

1. DIRECTED by the principles of the theory which has been explained (Prob. I. II.) I have been led to adopt the following general rules for conducting the cure of these fevers.

SECTION I.

Of the general Rules for curing putrid intestinal remitting fevers, without local affection.

ART. I.

In their mild and safe forms.

XLIII.

1. In our account of the febrile state, we had occasion to observe, first, that in putrid intestinal remitting fevers, the mucus which lines the intestines, especially the smaller, is in a putrifying state; and that this being absorbed and introduced into the blood in circulation, becomes the exciting cause of that remarkable change which constitutes the febrile state;

and, secondly, that the contagious matter which infects and corrupts the mucus, requires a considerable interval to produce these effects; and may therefore be thrown out of the body by an early exhibition of emetic, purging, and sudorific medicines, before the disease is confirmed.

2. For these different purposes; *tart. emetic.* owing to the expedition and certainty with which it operates, when properly managed, is better calculated than any other medicine we know; and upon this foundation we establish our first general rule for curing these fevers.

RULE I.

“That the contagious matter must be evacuated, if possible, before the mucus of the stomach and intestines be infected and corrupted; or before an absorption sufficient to excite and confirm the disease has taken place, by vomiting, purging, and sweating, with the forms of *tart. emetic.* that are best suited to these several purposes.”

XLIV.

1. With respect to the febrile state, we observe farther, first, that when it is once confirmed, it is always, as far as we have been able to discern, in proportion to the presumable quantity and acrimony of the matter absorbed from the intestines; that is to say, that the violence of the febrile state is constantly increased when the contents of the bowels are long retained, and become highly offensive, and constantly prevented from increasing when they are maturely and effectually evacuated. And we observe, secondly, that in putrid intestinal remitting fevers, (and I suspect in all others,) there is a constant tendency to costiveness, with evident signs of a spasm or constriction of the bowels, which increases with the other febrile symptoms, during the meridional paroxysms, and remits during the inter-meridional intervals, especially that which happens in the morning; and that, during these remissions,

the operation of purging, laxative, and sudorific medicines, is certain, expeditious, easy, and effectual ; but quite otherwise during the continuation of the meridional paroxysms.

2. To these observations, I have here to add, *first*, that *calomel*, in a degree superior to any other medicine I am acquainted with, possesses the property of loosening and detaching the mucus of the intestines ; that it requires, in general, from 6 to 10 hours to effect this perfectly, and operates best when the patient lies quietly a-bed, without tossing or moving about ; that a proper dose to an adult is from 6 to 12 grains ; and that this dose must be repeated from 3 to 6 times * at the beginning of the disease ; and afterwards, occasionally, as it may seem to be required by the fulness of the bowels, and state of their contents. We have to add, *secondly*, that, in discharging the mucus, when loosened and detached by the *calomel*, together with the other faeces, a solution of *sal. cathart. amar.* with *crem. tart.* and a very small proportion of *tart. emetic.* given in divided doses every hour, or every half hour, operates more certainly, expeditiously, easily, and effectually, than any other medicine I have used. Three or four small doses must be given every morning after the doses of *calomel*, at the beginning of the disease, and afterwards in the course of it, as many every morning, or every second morning, as are sufficient to keep the bowels perfectly open and easy.

Upon these premises, we establish our second and third general rule, to be observed in the curing of these fevers.

RULE II.

“ That, when the first attempts† to stop the fever at the beginning, by immediate evacuations, by vomiting, &c. with

* This must be determined by the state of the stools.

† These attempts must be made, if possible, in the course of the first day, before night, so as not to prevent the exhibition of the *calomel* at bed-time.

“ tart. emet. have been made; and when it appears to be
 “ established and confirmed, the time of the nocturnal me-
 “ ridional paroxysms must be dedicated, during the four
 “ or five first days of the fever, to the task of loosening and
 “ detaching the mucus from the coats of the intestines, by
 “ repeated doses of calomel taken at bed-time; and these
 “ must likewise be exhibited, for the same purpose, during
 “ the course of the disease, whenever they appear to be
 “ required by the state § of the bowels.”

RULE III.

“ That the time of the succeeding morning inter-meridional
 “ intervals must be employed, during the four or five first
 “ days of the fever, in discharging the mucus which has
 “ been loosened and detached by the doses of calomel,
 “ with whatever other faeces have been collected in the
 “ course of the night, by the laxative solution of sal. ca-
 “ thart. amar. &c. of which small doses must likewise be
 “ continued every morning, or every second morning, in
 “ the course of the disease, to prevent the collection and
 “ remora of any putrid and offensive matter, and its future
 “ absorption.”

XLV.

1. Whilst we are taking these steps to prevent the absorption of the putrid matter from the intestinals, it is also necessary to remember, that the patient's linen and bed-clothes, together with the air which surrounds him, will become impregnated with *contagious effluvia*, no less capable of being admitted into the body, and of exciting and supporting fever, than the original infection; and from this consideration we are directed to a fourth general rule.

RULE IV.

“ That every possible means must be employed to prevent

§ That is to say, their fulness, and the offensiveness of the stools.

“ the re-admission of contagious effluvia into the body, by
“ renewing and correcting the air in which the patient
“ breathes, &c. and by a proper change of his linen and
“ bed-clothes.”

XLVI.

1. A mixture of farinaceous substance, with the juice of fresh fruit, seasoned with wine, sugar, and nutmeg, or cinnamon, in the form of *panada*, makes a nourishment that is, in general, more refreshing and acceptable to the patient than any other, and well calculated to counteract the putrid tendency of the disease. The most proper time for exhibiting nourishment is in the course of the day, after the operation of the morning laxative, and before the approach of nocturnal exacerbation.

Upon this we establish a fifth general rule, to be attended to in curing these disorders.

RULE V.

“ That the body must be refreshed and nourished, particular-
“ ly in the course of the day, after the operation of the
“ morning laxative, and before the approach of the nocturnal
“ exacerbation, by small cupfuls of *panada* given at
“ proper intervals.”

XLVII.

1. It was observed before, in its proper place, that the natural tendency of these fevers is to make their attack, to increase in their violence, and to continue during the lunar periods; and, on the other hand, to defer their attack, to abate in their violence, and to terminate in the interlunar intervals: that many terminate in the first interlunar interval; but that the more general tendency seems to incline them to run on to the second; that, beyond the second, they continue, in

some cases, going on to interlunar intervals still more distant, even to the 7th; and that there is reason to believe that they proceed occasionally to intervals yet more remote.

2. But, although it be the natural tendency of these fevers to increase in their violence during the lunar periods, and although they sometimes continue for an extraordinary length of time, it is to be remembered, that, if the general rules I have recommended for their cure be properly observed, the symptoms very seldom arise to any alarming height; and that they generally terminate successfully in the first and second interlunar intervals.

From these reflections, there ariseth a sixth, and a seventh general rule, to be observed in curing these fevers.

RULE VI.

“ That, being assured that the lunar periods will certainly aggravate the meridional paroxysms, we are on no account
 “ to procrastinate, at the beginning of the disease, the exhibition of the medicines prescribed for clearing the
 “ bowels, from any idle hope that it will vanish of its own accord; but, on the contrary, are to proceed with all the
 “ expedition we can; and are likewise to be careful to avoid every irregularity that may increase the violence of
 “ the fever, and concur with the natural tendency of this
 “ period.”

RULE VII.

“ That we are not to be alarmed by a continuation of the
 “ fever, or by any moderate aggravation of the symptoms
 “ that may take place at the lunar periods; but to proceed
 “ steadily, according to the method we have proposed,
 “ without changing our plan, or harrassing our patient
 “ with a constant change and trial of unnecessary and pain-

“ful remedies ; knowing that all will terminate well, and
“in good time, in one of the interlunar intervals.”

XLVIII.

1. From the different remarkable changes which take place in the symptoms at the final interlunar crisis, viz. a sediment or a turbid appearance in the urine ; a more free and natural perspiration ; spontaneous stools ; a clearer, moister, and softer tongue, with a more free and natural discharge of saliva ; a more loose and copious expectoration ; a free discharge of bile, which seems to disappear, and to be suppressed, in the course of the fever, &c. &c. we conceive, I have already said (XXXII. 4.), that two important events take place upon this occasion : *First*, that the spasm excited by the putrid particles introduced into the blood, now begins to give way ; and, *secondly*, that the excretories becoming, by the solution of the spasm, more open and relaxed, and the secretions more free and copious, the corrupted particles themselves will also be discharged in greater quantity ; and, from the concurrence of these two events, we are led to infer that the fever is brought, sometimes suddenly and at once, and sometimes gradually and slowly, to a final and happy termination.

2. Proceeding upon this theory, I am confident that I have contributed greatly to bring the fever to a complete and speedy crisis, by using means to co-operate with the natural tendency of the interlunar interval to resolve the spasm ; and, by encouraging and encreasing the secretions which naturally incline to take place at this juncture. To effect these two purposes, on the morning which succeeds the expiration of the lunar period, I take care to keep the patient exceedingly quiet ; and to remove, at the same time, every thing in the bowels that may possibly support irritation and spasm ; and in the course of the day, after the laxative has operated, I encourage a gentle perspiration, by an antimonial, or by other means that may be judged more proper.

From this discussion, we obtain an eighth general rule.

RULE VIII.

“ To co-operate with the natural tendency of the commencement of the interlunar interval to resolve the spasm ; and
 “ to encourage the discharge of the morbid particles circulating in the blood, by the natural secretions, and to
 “ act upon this plan during the whole of the interlunar intervals.”

XLIX.

1. If, at the end of the interlunar interval, the pulse, notwithstanding all these endeavours, should continue quicker than its natural standard, the tongue foul, and the sleep disturbed by dreams in the night ; and if, instead of a proper sediment at the bottom of the glass, the urine should exhibit a suspended mucus-like cloud, although the patient should be pretty well in other respects, and should have even recovered some appetite for food, there is great reason to apprehend that the seeds of the fever are still lurking within ; and that it will again appear in its proper colours, in the course of the succeeding lunar period.

These observations suggest a ninth and tenth general rule.

RULE IX.

“ That, if the symptoms of the febrile state have not perfectly disappeared at the end of the interlunar interval, we
 “ are to be on our guard to avoid every irregularity that
 “ may concur with the natural tendency of the lunar period to renew the fever.” And,

RULE X.

“ That, if the fever should revive in the lunar period, it is
 “ to be treated upon the principles and rules we have explained ; making proper allowance for the change that
 “ has taken place in the patient’s strength, &c.”

L.

1. These are all the rules which seem to be required for the cure of putrid intestinal remitting fevers, when they appear in forms that are *really mild* and *safe*. And, as the medicines prescribed by these rules do not suddenly, like the peruvian bark, interrupt the fever at once, but allow us to observe it proceeding in its natural course and direction, I have often been induced, from this motive, to depend on these alone, in cases which appeared to be sufficiently *mild* and *safe*; and where I did not entertain any apprehension of danger. But, as I have been more than once deceived in cases of this kind, and as the method of giving the *bark* with opium, in the manner recommended for the cure of the *more dangerous forms* of these disorders, gives great security, and can be attended with no inconveniency to be put in opposition to this advantage, I recommend it earnestly to every young practitioner to proceed upon the supposition, that none of these fevers, however mild their appearance, are free of danger; and to treat them, in every case, with the *bark*, at the beginning, according to the rules laid down for curing them when they appear in the forms that are considered as *more violent* and *dangerous*, (Vid. LI.)

ART. II.

In their more violent and dangerous forms.

LI.

1. Having delivered the general rules that are to direct us in treating the milder and safer forms of putrid intestinal remitting fevers, without local affection, we now proceed to provide rules for the management of those which are more violent and dangerous.

2. In the *milder forms*, when they are treated in the manner we have directed, the heat, anxiety and debility, continue moderate; and although the patient's head may be somewhat deranged whilst he is awake, and his slumbers disturbed with

disagreeable dreams, he still, however, continues to get rest, and to be in some degree refreshed.

In such cases, we infer, from the moderation of the symptoms; First, that the putrefaction going on in the mucus of the intestines is slow and inconsiderable; and that there is no danger of the intestines themselves being affected with mortification: Secondly, that the quantity and irritating nature of the corrupted particles which have been absorbed are likewise inconsiderable; and that it is capable of producing mild meridianal paroxysms only, which do not injure the brain by their violence; and are not attended with danger: And, lastly, that the contents of the bowels being carefully evacuated, and prevented from being absorbed, and what is already absorbed constantly carried off by the skin, kidneys, &c. the fever will at last terminate of its own accord, at one of the intervals, without the aid of any other medicines than those which we have prescribed.

3. But it is otherwise in *the more violent and dangerous forms* of these fevers which are the subject of our present consideration; for, in these, the heat, anxiety and debility, are excessive; and the head being deranged to an alarming degree, and the patient deprived entirely of rest, we are then led to conclude, not only that there is danger of a mortification of the bowels from the degree and extent of the putrefaction going on in the mucus; but that the matter already absorbed, by continuing to excite violent meridianal paroxysms, may cause a determination towards the head inconsistent with life*.

* I have been fortunate enough to meet with very few cases in which the more violent forms of these fevers have proved fatal: but, in almost all of them, life seemed to be destroyed by the injury done to the brain during the violence of the paroxysms; and not by any general putrefaction or dissolution of the fluids, or of the solid parts.

In some cases, the whole powers of life seemed to be entirely overpowered

For these reasons, we are under the necessity of applying to other remedies besides those which we have prescribed for milder cases, in which we apprehend no danger.

4. To procure immediate relief from the excessive anxiety and restlessness of putrid fevers, there is no remedy so powerful and certain as *opium*; and we refer its effects to the power it possesses of destroying the sensibility and excitability of the system; and of thus rendering it, in a great degree, defended against the action of the morbid particles circulating with the blood; to which cause we are inclined to ascribe the violent spasmodic affections of the vascular system, which produces that dangerous determination to the head in the course of the meridional paroxysms we so much dread.

5. To prevent the putrefaction of the mucus, and the mortification it may induce, and also to put a stop to the meridional paroxysms, we are acquainted with no remedy so powerful and certain as the *peruvian bark*, especially in substance. After the experiments † that have been made upon it, it cannot be doubted that it possesses the property of correcting the putrefaction of the mucus; and that, by being applied immediately to the bowels themselves, it will likewise prevent their mortification: And its power of suppressing the meridional paroxysms, by some mode of acting not so easily explained, is no less certain. Perhaps it may act by entering into the blood, and correcting the nature of the putrid particles; which probably produce the spasmodic state, by pervading and irritating the whole vascular system:—Or, perhaps, the spasm-

and destroyed at once, as if it were by a poison; and, in all these cases, there was great reason to suspect a large absorption of putrid matter from the intestines. When putrefaction takes place in other parts of the body, by which life is destroyed suddenly, it is probably by an absorption of putrid matter from these parts operating in the same way.

† Vid. Alexander's Experiments, &c. &c. &c.

dic state may depend more particularly upon the putrid particles being allowed to enter into the minute vessels of the brain; and perhaps the bark produces its salutary effects, not only by correcting the putrid particles, but by strengthening and contracting the vessels of the brain, so as to prevent them from being admitted.—But, in whatever mode the bark produces its effect, it ought to be considered as an infallible security against danger in these fevers, when given in sufficient quantity. For, although it may fail to stop the meridianal paroxysms during the first lunar periods‡, even when given in large quantities, for several days, yet let it not, on this account, be distrusted or discontinued; for, in the space of 20 years, I cannot say that I have met with any case, in which I conceived it to be properly administered, and in sufficient quantity, where it ever failed of securing the patient in the end.

From these premises, therefore, *respecting the cure of putrid intestinal remitting fevers without local affection, in their more violent and dangerous forms, we are led to the following general Rules.*

RULE I.

“ That the rules already proposed, in treating the mild and
 “ safe forms of these fevers, for stopping the fever, if pos-
 “ sible, in the very beginning§ : for evacuating the bow-

‡ Several late observations made in India, since I came to be better acquainted with the theory of sol-lunar influence, incline me to suspect that this is generally the case in the more violent forms of the putrid remitting fevers of that country; and that many instances of sudden and complete *final crises*, which were ascribed to the virtue of the *bark alone*, were in part due to the decline of sol-lunar influence, concurring to assist it at the commencement of the interlunar interval.—And, perhaps, it may be discovered hereafter, that many other wonderful and unexpected *crises*, in other disorders attended with fever, (in the *dropsy*, for example,) attributed at this day to the power of certain medicines, have been really owing to the same cause.

§ By tart. emet.

“els effectually with calomel and the laxative solution;
 “and for giving nourishment, are to be observed strictly
 “during the two first days of such forms as may threaten
 “to be violent and dangerous: That, on the morning of
 “the third day, immediately after the operation of the
 “morning laxative, in order to check putrefaction, suppress the violence of the meridional paroxysms, and give
 “security against a failure of the strength, or any unfavourable turn in the fever, the bark is then to be exhibited in substance, and to be continued || for two days,
 “in such doses as to throw in at least 12 drams or 2 ounces before the expiration of the second day: That then
 “the calomel is to be repeated at bed time, and the laxative in the morning: That immediately after the operation of the morning laxative, the bark is again to be reiterated for two days, just as before: That the calomel and laxative are again to be repeated; and so on, until
 “the fever give way; after which, it is sufficient to support the effect of the powder by a few doses of the decoction given daily, and to repeat the laxative solution every second or third day, as occasion may require.

RULE II.

“That, to prevent the bark from being thrown up, or passed by stool, and also to make nourishment sit upon the stomach, and to procure ease and repose, opiates are to be given freely.

1. By observing these rules, I have treated the putrid fevers

|| In 1769, very soon after my arrival in India, I was fortunate to discover the wonderful efficacy of the bark in substance in curing the putrid intestinal remitting fever of Bengal, commonly called the *pucca fever*; and at the same time, the perfect safety of giving it during the height of the meridional paroxysms, whether nocturnal or diurnal; and consequently the great danger and *infatuation* of waiting for a spontaneous remission of the fevers. The bark may be given in wine, or wine and water, just as the strength may seem to require it. I used to give it in claret without limitation; and with the best effect.

of India with great success; and we conceive that, with a little variation, they will be found applicable to every form of these disorders attended with danger.

SECTION II.

Of the general rules for curing putrid intestinal remitting fevers with local affection.

ART. I.

In their mild and safe forms.

LII.

1. In treating of the arrangement of putrid intestinal remitting fevers, in the beginning of this dissertation, it was observed, that all the disorders we had met with in India, under the form of dysentery, and most of those under the form of pleurisy, peripneumony, acute rheumatisms, and many other local affections attended with fever, were nothing more than so many different cases of putrid intestinal remitting fevers accompanied with different local affections; and therefore to be distinguished from each other in this respect alone. Agreeably to these ideas, we arranged all putrid intestinal remitting fevers under the two following divisions, viz.

1st. *Putrid intestinal remitting fevers without local affection;*

2d. *Putrid intestinal remitting fevers with local affection.*

And we concluded, that the proper method of treating all putrid remitting fevers was exactly the same; except in those respects where their different local affections require remedies peculiarly suited to their nature.

2. Upon this general principle, we therefore proceed now to consider more particularly the treatment of putrid intestinal remitting fevers with local affection, which we subdivide likewise into two classes, viz.

a. *Those that are mild and safe.*

b. *Those that are more violent and dangerous.*

3. In the milder and safer cases of putrid intestinal remitting fevers with local affection; whether dysenteries, pleurisies, peripneumonies, or rheumatismus, &c. the treatment prescribed for the mild and safe cases of putrid intestinal remitting fevers without local affection, has been found to be perfectly sufficient and proper; without any other addition than that of keeping the whole body, and especially the parts affected, better defended from cold, and of using other means to produce and support a gentle perspiration.

We therefore propose the following general rules *for the cure of the safe and mild forms of putrid intestinal remitting fevers with local affection.*

RULE I.

“ That they are to be treated exactly according to the general rules laid down for treating the mild and safe forms of putrid intestinal remitting fevers without local affection.”

RULE III.

“ That the whole body, and especially the parts affected are to be better defended from cold, than when there is no local affection; and other means used to promote and support a gentle perspiration.

LIII.

1. These are all the rules that seem to be required for curing putrid intestinal remitting fevers *with* local affection, when they appear in forms that are *really safe and mild*. But as I have been deceived in cases of this kind, and as the method of giving the bark in the manner recommended for the cure of the more dangerous forms of putrid intestinal remitting fevers *with* local affection, (Vid. IJ. and Rule I. II. and III.) gives great security, and is attended with no inconvenience to be put in opposition to the advantage; I here, also, recommend it

earnestly to every young practitioner, to proceed upon the supposition, that none of these fevers, however mild their appearance, are free of danger, and to treat them, on every occasion where there is the smallest doubt, as nearly as the circumstances will admit, according to the rules laid down for curing them when they appear in forms that are more evidently violent and dangerous.

ART. II.

In their more violent and dangerous forms.

LIV.

1. Considering the infectious nature of these fevers, it is probable that no constitution whatever is secure or exempted from their attacks; and that they are therefore often produced in the highest degrees of inflammatory diathesis, where there does not exist the smallest tendency to any putrid disorder; and of this I think I have seen many instances in the course of my practice. But however phlogistic the constitution, I have very seldom and almost never, judged it necessary in India, *to draw blood when there was no local affection*; and have found it sufficient to proceed according to the rules laid down for treating the more violent and dangerous forms of putrid intestinal remitting fevers without local affection. On the other hand, *when the symptoms of inflammation fixed themselves violently and obstinately on any particular part, I have never hesitated* (at the beginning of the disease,) *to use the lancet*; especially if they did not yield to the remedies that are recommended at this time. The loss of blood, both general and local, is very effectual in removing these partial affections; and when it fails, it prepares the way for a free exhibition of opium to alleviate the pain, and of bark to suppress the fever; by the violence of which, these partial affections are often supported and aggravated, more than by any topical cause. When the local affection proves so obstinate as to resist all ordinary means, *blisters* are then applied to, and produce often surpris-

ing effects. But, since I began to regulate my practice by the rules which I have so fully explained, I have seldom had occasion to employ them; and I have considered it as an improvement of some consequence, independent of its success, to be able to avoid so painful a remedy.

From these reflections we deduce the following general rules for curing the more violent and dangerous forms of putrid intestinal remitting fevers with local affection.

RULE I.

“ That they are to be treated exactly according to the general rules laid down for curing the more violent and dangerous forms of putrid intestinal remitting fevers without local affection.”

RULE II.

“ That the whole body, and especially the parts affected, are to be more carefully defended from cold than when there is no local affection; and other means used to promote a general perspiration.”

RULE III.

“ That bleeding and blistering are to be administered when indicated by the violence and obstinacy of the local affection; especially when it is seated in any vital or important part; taking care to obviate the failure of the general strength which is apt to succeed the loss of blood, by an immediate exhibition of the bark, wine, and nourishment.”

SECTION III.

Of the variation required in the treatment of putrid intestinal remitting fevers, by the changes produced in their nature and tendency at the equinoctial periods, and inter-equinoctial intervals.

LV.

1. Under this head, I have only to say, in general terms,

that the variation which we have spoken of, at the equinoctial periods, particularly the autumnal, and its respective inter-equinoctial intervals, seem to suggest no other alteration in the mode of treating putrid intestinal remitting fevers at these times, except, perhaps, a *more* or *less* expeditious and vigorous application of the means we have already recommended in the foregoing rules.

The following application of these general observations to the cure of a putrid intestinal remitting fever, *with* an affection of the bowels, or, in other terms, a *dysentery*, is here annexed, because it has been found to be singularly successful in a disorder, which is, above all others, destruetful to Europeans in warm climates; and may, at the same time, serve in some degree, to illustrate the tenor of our practice in every kind of putrid intestinal remitting fever, whether *with* or *without* local affection.

I.

℞ Tart. emet. gr. iv. solve in
 Infus. fruct. tamarind. eum man. lib. ij. et

Sign. The emetic solution to be used instantly upon the first appearance or apprehension of the disorder, and to be given in small wine glassfulls, every half hour, until it has operated freely, both upwards and downwards.

II.

℞ Calomel. gr. viii.
 Opii pur. gr. ii.
 Syr. eoīs. q. s. u. f. pil. No ij. et

Sign. A dose of calomel to be given at bed time (8 o'clock) on the first day of the disorder, and to be continued for 4 or 5 nights following, or longer, if the bilious and putrid nature of the stools should seem to require it; and

to be repeated at any time in the course of the disease, when judged necessary *.

The opium is united with the calomel, not only to make it sit on the stomach, but to procure rest. The quantity may therefore be increased at pleasure, to secure this effect, and the opiate must be continued every night after the calomel is intermitted.

III.

℞ Sal. cathart. amar.	ʒ x.
Crem. tart.	ʒ ss.
Aq. font.	lib. ii. et adde
Tart. emet.	gr. i. M. et

Sign. The laxative physic, of which a wine glassful is to be given every day, from the first attack of the disease, until it begins to give way, at 5 in the morning, and continued every hour until it has operated freely. After the symptoms have begun to abate, it will be sufficient to keep the bowels gently open with smaller doses of the same medicines. But this must be done in the morning daily, through the whole course of the disease.

Castor oil, or any other laxative more agreeable to the patient, may be substituted occasionally. But castor oil is, above every medicine, useful, when there is much tenesmus and

* In common cases of the remitting putrescent fever, without dysenteric symptoms, I omit the opium; and for several years past, it has been my practice to give from 5 to 8 grains of calomel, with an addition of 8 or 10 grains of extractum catharticum, or some such purgative, for a dose, to be repeated every night, or rather every other night, for four or five times, at the beginning of the disorder. And, as the opium, I suspect, interferes with the operation of the calomel, this purgative should likewise be preferred at the beginning of dysenteries, where the gripes, &c. are not severe and troublesome; and where the addition of opium is not absolutely necessary for giving the patient some respite in the night.

signs of great constriction on the bowels, and then an opiate should be given previously, to make it sit on the stomach.

IV.

R Laud. liquid. gtt. XL.

Aq. menth. pip. ζ i. m. f. haust. et

Sign. The quieting draught* to be given every day about *mid-day*, after the evacuation of the bowels has been well affected by the morning laxative; and the dose to be made sufficient for keeping the patient easy, and the bowels in a quiet state, until the time of taking the second opiate at bed-time.

When the rectum is much inflamed, emollient glysters give great relief; and, if the opiates prescribed should fail in their effect, an additional dose may be given in this form. And let it be recommended strongly to every patient labouring under a dysentery to force himself away from the chair the moment that the faeces which occasioned the call are evacuated. The straining that follows is always fruitless, and tends only to inflame the rectum still more. But, as the erect posture always increases the tenesmus, the best method of all is to receive the faeces upon clothes introduced below the patient, for that purpose.

V.

In the course of the day, the patient must be frequently nourished with panada, made in the manner recommended in the treatise, and also in the night, if it should be necessary. Gruels, made of rice or oatmeal, are the most proper drink.

* In remitting putrescent fevers of every kind, there is no medicine so cordial and sedative as opium. In those unattended with dysenteric symptoms, I have generally contented myself with giving it in smaller doses than is here prescribed, along with the bark or snake-root. But, in all cases, the dose should be made sufficient to remove the restlessness which accompanies these fevers, and to compose.

VI.

When the fever does not give way in three days to the above treatment,

℞ Decoct. cort. peru. fort. lib. ij.

Laud. liquid. gtt. XL. M. et

Sign. The decoction †, of which give 3 ounces every hour, beginning after the mid-day opiate has settled the stomach, and continue taking it through the night when awake.—When the fever is not alarming, bark is not absolutely necessary to the cure, but it gives great security in every case of fever and flux.

☞ Having found it necessary when publishing this Collection of Treatises in Bengal, a few years ago, to introduce in the Preface, a discussion relating to *the practice of using mercury as a febrifuge, in large and unlimited quantities*, that discussion is now referred to here, as a *supplement* to the preceding practical rules.

† Whenever I am anxious to suppress the fever immediately, I trust to the bark in substance only; and when this is the case, the purgative and laxative medicines must be omitted for a day or two, until that be effected. I have prescribed the decoction in this treatise only because it will be in general more acceptable to dysenteric patients, whose stomachs are delicate.



TREATISE III.

ON

THE ACTION

OF

SOL-LUNAR INFLUENCE.

DEDICATED

TO

COLONEL NESBIT BALFOUR,

AND TO

MAJOR ARTHUR BALFOUR,

AS A TESTIMONY

OF ESTEEM.

P R E F A C E.

IN the induction by which we established our AXIOMS * respecting the laws of sol-lunar influence, we found it necessary to set out with certain general facts, comprehended under the general term of *symptoms*.—In order to enable other inquirers to trace the course of our reasoning to its remotest source, we have now undertaken to exhibit, by a minute ANALYSIS of these AXIOMS, the first and most simple phaenomena with which our observations originated, and particularly the *appearances of the urine*; from which we have been led to inferences important in medicine, and philosophy in general.

* Vid. Treatise II. page 86.

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ON
THE ACTION
OF
SOL-LUNAR INFLUENCE.

CHAPTER I.
THE INTRODUCTION TO THE ANALYSIS.

SECTION I.

The truth of the axioms advanced respecting sol-lunar influence, to be ascertained by recurring to the first and most simple phænomena with which our induction commenced.

I.

1. A CERTAIN spasmodic state excited by infection, and regulated in its revolutions by sol-lunar influence, conveys an idea comprehending the whole theory of infectious fevers.

The doctrine of the febrile spasmodic state, first delivered by Hoffman, and afterwards more fully explained and diffused by Cullen, and his scholars, seems, with certain limitations, to be almost universally adopted by the present age.

The nature and extent of febrile infection, hitherto only anticipated and assumed upon partial observation, is now placed upon the basis of an extensive induction, by the indefatigable and judicious industry of Doctor Robertson †.

† Vide an Essay on Fevers, wherein their theoretic genera and species, and

The laws of sol-lunar influence formerly superficially observed by others, have been explained in our treatise on putrid intestinal remitting fevers, in a manner, we presume, not to be found in any preceding record of medicine.

2. Reconciling and combining these different principles, I formed the system of theory and practice which was offered to the public in that treatise.

II.

1. In Europe, the theory of sol-lunar influence has met with little encouragement from the professors of medicine. Those who allow that it may prevail in India, nevertheless insist that it does not shew itself in the northern latitudes of this quarter of the globe.

If the physicians who have delivered this opinion can declare that they have watched the state of the symptoms, and examined the urine, at the coming on, in the middle, and at the going off, of the different paroxysms and remissions, during the whole course of the disease; that they have likewise particularly attended to the character of the symptoms and urine, at the coming on, in the course, and at the going off, of the different lunar periods, and interlunar intervals; that they have continued these observations, in an extensive practice, for a length of time; and that, after all, they have been able to dis-

various denominations, &c. &c. are reduced under their characteristic genus, febrile infection, by Robert Robertson, M. D.

The discovery of the laws of sol-lunar influence led me to infer, that all the various forms of remitting infectious fevers were nothing more than so many varieties of the same disease, occasioned by that influence exerting itself under different circumstances; and to anticipate the conclusion which Dr Robertson has established upon direct observation, long before I was acquainted with his opinion on this subject: and he will perceive that it is a conclusion which unavoidably arises from these premises.

cover no coincidence or connection similar to that which we have described ; we shall then allow that they have formed their opinion upon reasonable grounds, and have been guilty of no impropriety in giving it to the public :

If, on the other hand, this opinion has been formed from observations collected in the course of the ordinary visitations that are paid to the sick in this country ‡ by their physicians, whether in hospitals or in private practice, I must then affirm, that it has been advanced upon grounds that are in every respect imperfect and insufficient. But, what is worse, it is an opinion which is prejudicial, because it *insidiously* tends to prepossess the mind of *the young student*, and to lead it astray from the investigation and discovery of truth.

2. To resist and prevent this consequence, as far as it may be done by the voice of a single individual, I think it a duty, which I owe to mankind, to declare, in the most public and solemn manner I am able, that since I came to Scotland, I have had an opportunity of attending to more than twelve different cases of bilious fever ; and that, in all of them, I discovered an evident coincidence of their daily and septenary revolutions with those of the moon, similar to that observed in India. In several of these cases I had an opportunity of observing *the changes of the urine* with attention, as well as *the other symptoms*. In the rest, I formed my opinion from *the other symptoms only*.

III.

1. It is the first object of this treatise to point out a regular and simple method of investigation, by which the facts and arrangements, from which the doctrine of sol-lunar influence has been inferred, may be readily traced and examined ; so that the foundation of this theory may be brought to a trial by every enquirer, and determined to be true or false.

‡ Great Britain.

CHAPTER II.

THE ANALYSIS.

SECTION I.

The first and most simple phaenomena with which our induction commenced to be exhibited by following a retrograde or descending course.

IV.

1. INVESTIGATING the doctrine of sol-lunar influence, we proceeded by a progressive or ascending scale of observation. It is our present purpose to examine it by a scale of observation which retrogrades or descends. By this means we shall be conducted to the first and most simple phaenomena with which our observations began ; and upon the reality of which the truth of the whole system we have endeavoured to establish, must ultimately rest.

2. In prosecuting the ascending mode of investigation, we obtained three general rules or AXIOMS in which our ideas of the general laws of sol-lunar influence are expressed ; with certain clauses explaining in what respects these general laws seem to be modified or restrained. By exhibiting these axioms analysed and reduced to the different subordinate propositions on which they depend, and these again to the most remote principles or phaenomena from which they are inferred, we shall perform the task we have undertaken, and in the manner we proposed.

The axioms that we obtained by our first investigation are these :

AXIOM I.

“The force of sol-lunar influence is much greater during the
“ meridional periods than their respective inter-meridional

“ intervals ; and seem to be somewhat greater during the
 “ nocturno than their respective diurno-meridional peri-
 “ ods ; and somewhat greater during the evening than the
 “ morning inter-meridional intervals.”

AXIOM II.

“ The force of sol-lunar influence is much greater during the
 “ lunar periods than their respective interlunar intervals ;
 “ and during the novilunar and plenilunar periods it is
 “ nearly the same ; and also during their respective inter-
 “ lunar intervals, at the beginning and end of which it
 “ seems to be greater than in the middle.”

AXIOM III.

“ The force of sol-lunar influence is considerably greater du-
 “ ring the equinoctial periods than their respective inter-
 “ equinoctial intervals ; and somewhat greater during the
 “ autumnno than the verno-equinoctial periods.”

THE ANALYSIS.

I. AXIOM. Contains the following propositions :

1. The force of sol-lunar influence is much greater during the meridional periods than their respective inter-meridional intervals ;—inferred,

A. From the prevailing tendency of the febrile paroxysms to coincide with the meridional periods ;—inferred,

A. From the superior number of instances in which the febrile paroxysms were actually observed to coincide with the meridional periods ;—inferred,

a. From the presence of that superior degree of the principal symptoms which distinguishes the character of the fe-

brile paroxysms, viz. the pulse more quick and hard than in the remissions, the skin more hot and dry, the tongue more dry and parched, the expectoration more scanty, the belly more bound, the face more flushed, the eyes more confused and red, the head more deranged, the sleep more interrupted and disturbed, the pain of partial affections more severe, &c. &c. and, above all,

b. From the tendency of the urine to be *more thin* than in the remissions ;—inferred,

aa. From being *less mucose, nubiferous, nubipulviferous or pulviferous*, than in the remissions. Vide the Definitions that follow under par. VIII. and IX.

B. From the prevailing tendency of the febrile remissions to coincide with the inter-meridional intervals ;—inferred,

A. From the superior number of instances in which the febrile remissions were actually found to coincide with the inter-meridional intervals ;—inferred,

a. From the presence of that inferior degree of the principal symptoms which distinguishes the character of febrile remissions, viz. the pulse less quick and hard than in the paroxysms, the skin less hot and dry, the tongue less dry and parched, the expectoration less scanty, the belly less bound, the eyes less confused and red, the head less deranged, the sleep less interrupted and disturbed, the pain of partial affections less severe, &c. &c. and, above all,

b. From the tendency of the urine to be *less thin* than in the paroxysms ;—inferred,

aa. From being *more mucose, nubiferous, nubipulviferous, or pulviferous*, than in the paroxysms.

2. And seems to be somewhat greater during the nocturno-meridional than their diurno-meridional periods ;—inferred,

A. From the prevailing tendency of the nocturno-meridional paroxysms to make their appearance at an earlier stage of the fever, to be more violent, and to continue their returns after the diurno-meridional paroxysms have ceased to appear ;—inferred,

A. From their having been actually observed in most cases, since I began to attend to this circumstance, to make their appearance at an earlier stage of the fever.

B. From having always observed that the symptoms in most cases were more severe.

C. From their being actually observed to continue their returns in the course of the interlunar intervals, long after the diurno-meridional paroxysms have ceased to appear.

B. From the prevailing tendency of the diurno-meridional paroxysms to make their appearance at a later stage of the fever, to be less violent, and to continue returning for a shorter time ;—inferred,

A. From their having been actually observed in most cases, since I began to attend to this circumstance, to make their appearance at a later stage of the fever.

B. From their having been actually observed to be less severe in most cases.

C. From their having been actually observed to disappear, or at least to become more obscure, in the interlunar intervals, whilst the nocturno-meridional paroxysms continue to return with considerable force.

3. And somewhat greater during the evening than the morning inter-meridional intervals ;—inferred,

A. From the prevailing tendency of the morning inter-meridional remissions to be more complete than those of the evening ;—inferred,

A. From observing the symptoms of the morning inter-meridional remissions in every case more distinct than those of the evening.

B. From the prevailing tendency of the evening inter-meridional remissions to be less complete than those of the morning ;—inferred,

A. From observing that the symptoms of the evening inter-meridional remissions were always less distinct than those of the morning ; and from their becoming so obscure during the 3d or 4th last days of the lunar period, as to be scarcely observed ; but especially in the first lunar periods.

II. AXIOM. Contains the following propositions :

1. The force of sol-lunar influence is much greater during the lunar periods than their respective interlunar intervals ;—inferred,

A. From the following prevailing tendencies in the lunar periods :

A. The prevailing tendency of the meridional paroxysms of the lunar period to attack, to increase in violence, and to continue ;—inferred,

a. From having observed that the meridional paroxysms of the lunar periods in most cases made their appearance first.

b. From having observed that in most cases the meridional paroxysms of the lunar periods pitched, as it were, upon a

higher key, and were longer and more violent than in the interlunar intervals.

c. From having observed that the meridional paroxysms of the lunar periods, especially of the first and second, seldom or never ceased of their own accord, but, on the contrary, became more and more violent and obstinate, from the beginning of the period to the end.

B. The prevailing tendency of the inter-meridional remissions of the lunar periods to become, from the beginning to the end of the period, gradually less distinct, especially of the evening inter-meridional remission, which, about the middle of the period, is apt to become so obscure as not to be perceived ;—inferred,

a. From having observed that, in almost every case, particularly in the first and second lunar period, the symptoms of remission did actually become less distinct, especially those of the evening remission ; which become so obscure about the middle of the period as not to be observable.

B. From prevailing tendencies in the interlunar intervals, viz.

A. A prevailing tendency in the meridional paroxysms of the interlunar intervals to defer their attacks, to abate in their violence, and finally to terminate and disappear ;—inferred,

a. From having observed that the meridional paroxysms of the interlunar intervals seldom make their appearance first.

b. From having observed that, in most cases, the meridional paroxysms of the interlunar intervals, pitched, as it were, upon a lower key, and were shorter and less violent than those of the lunar periods.

c. From having observed that the meridional paroxysms of the interlunar intervals did, in most cases, become gradually less violent and shorter from the beginning of the interval to the end; or finally terminate and disappear.

B. A prevailing tendency in their inter-meridional remissions to be more perfect and distinct than those of the lunar periods, and to continue longer;—inferred,

a. From having observed, that, in most cases, the symptoms of remission were more perfect and distinct, and continued longer, than in the lunar periods.

2. And during the novilunar and plenilunar periods, it is very nearly the same; and also during their respective interlunar intervals;—inferred,

A. From a prevailing tendency in the symptoms of their paroxysms and remissions to be nearly the same;—inferred,

A. From having always observed the symptoms of their paroxysms and remissions to be nearly alike.

3. At the beginning and end of which, (the interlunar intervals) it seems to be greater than in the middle;—inferred,

A. From some degree of tendency in fevers to commence their attacks at the beginning and end of the interlunar intervals;—inferred,

A. From having frequently observed attacks both at the beginning and end of the intervals.

B. From there being little or no tendency in fevers to attack in the middle of the interlunar intervals;—inferred,

A. From having seldom, and almost never, observed fevers making their first attacks in the middle of the interlunar interval.

III. AXIOM. Having been inferred from general and imperfect observations, does not admit of a minute analysis. There is reason, however, to believe, that it has a real foundation in nature ; and we therefore recommend an attention to these experiments and observations by which it may be ascertained. Although we have fixed the limits of the equinoctial periods in an arbitrary manner, we are inclined to think that their influence extends beyond the bounds that we have prescribed*.

* By referring to Treatise VI. it will be found, that these anticipations have been confirmed in a very satisfactory manner by later observations.

CHAPTER III.

REMARKS ON THE ANALYSIS.

SECTION I.

Of the appearance of the urine in general.

V.

1. A FIXED and precise idea *of the appearance of the urine in fevers*, fit to guide the judgment respecting past, present, and future events, and to direct our practice in the cure of these disorders, we may venture to affirm is at this day a *desideratum* in medicine. The idea which I have been led to form, from long and attentive observation, appears to me to be fixed and precise. I think it has guided my opinion with certainty, and my practice with success; and it is therefore a principal purpose of the present essay to explain it to the public.

But, as the idea which we have given of the appearances of the urine in the preceding analysis, is too general for use; and, on the other hand, as it would be impossible to detail them in all the different forms of fever, without increasing this treatise to an immense size, which, after all, would be too various and multiform for the purpose of a practical rule, I shall now endeavour to illustrate the whole class *by one example*; which, if fixed and retained in the mind, may, with attention to a few variations, be readily applied to explain every case that may occur. Vide TABLE, XIII. 3.

VI.

1. It appears from the preceding analysis, that the truth of the different propositions contained in these axioms must rest ultimately on such observations as have led us to infer;

that the febrile paroxysms are always distinguished by a superior degree of violence or intenseness in the symptoms; and, above all, by the *urine* being *more thin* than in the remissions; and that the febrile remissions are always distinguished by an inferior degree of violence or intenseness in the symptoms; and, above all, by the *urine* being *less thin* than that in the paroxysms.

VII.

1. With respect to the *other symptoms* independent of the *appearances of the urine*, although the relative degree of their violence or intenseness is readily distinguished by a person of experience, and the presence of the paroxysms and remissions at any particular time is thereby determined at once, without hesitation or doubt in his own mind; yet the positive degree or height of none of the symptoms, except that of the pulse, and heat of the body, can be mathematically measured and recorded, so that their degree at one time may be mathematically compared with their degree at another: and the state, even, of the *pulse* and *heat*, is liable to variation from different accidents, and cannot always be assumed as a just representative of the real state of the fever. Owing to these circumstances, the opinion and report of every physician respecting the presence of paroxysms and remissions, and consequently respecting *their coincidence* with certain fixed periods of time, when inferred from these symptoms only, must be subject to mistake; and cannot afford the assurance or certainty of demonstration.

ART. I.

The different appearances of the urine observable in the course of fevers, seem to arise from particular changes in the state and proportion of the colouring, and of the gross matter: and these changes are most remarkable in the nocturno-meridional paroxysms, and morning inter-meridional intervals.

VIII.

1. Becoming, after the publication of my first Treatise on

the Influence of the Moon in 1784, more and more convinced of the truth of the theory I had advanced, and of the great extent of its application and importance, I became, in proportion, more and more anxious to make it known : and saw, with concern, the fallibility of the testimony arising from these symptoms alone, and the difficulty of conveying to others, with all its force, the combination of circumstances by which I was convinced myself. From this consideration, hoping that they would afford some evidence less equivocal, I was induced to pay more attention to *the appearances of the urine* than I had done before.

The symptoms of *the nocturno-meridional paroxysms and morning inter-meridional remissions* being the most remarkable, and more immediately contrasted with each other, drew my attention first ; and during these two periods *the changes* that took place in the state and proportion of *the colouring matter* ; and of *the gross matter*, under which I comprehend that which appears in the form of *mucus or powder*.

These different appearances are particularly described in the following DEFINITIONS ; previous to which it is necessary to observe, that as the urine in India, whether in health or disease, approaches either to a *red* or *yellow* colour, these terms will serve to express these two common distinctions.

In health it approaches generally to a yellow colour, unless when changed to red by exercise or motion, or any excess.

In fevers it soon changes to *red*, that is to say, to a colour resembling the different hues of porter or mahogany ; and in this sense the term *red* is to be understood.

1. *Red mucose urine*, which is red urine, exhibiting the appearance of mucous, or viscid matter, equally diffused through the whole of its substance, without any separate cloud or pow-

dery matter; and in the paroxysm generally resembling clear porter. Another term, perhaps, might have been found, more appropriate; but being thus defined it will answer the purpose at present.

2. *Red nubiferous urine*, which is red urine, exhibiting one or more mucus-like elouds suspended in it, and is of two kinds, or rather degrees, viz.

a. *Red nubilated urine*, so called when the cloud is single, and considerably large and opaque.

b. *Red nubeculated urine*, so called, when, instead of a single and compact cloud, there appear several thin light nubeculae.

3. *Red nubipulviferous urine*, which is red urine, exhibiting a combination of cloudy and powdery matter.

4. *Red pulviferous urine*, which is red urine, exhibiting a powdery matter suspended in it, which at first gives it a turbid appearance; and soon falls to the bottom, and forms a sediment, sometimes pale, and sometimes lateritious.

5. *Limpid urine*, is urine which exhibits no appearance of matter either colouring or gross, and is pure like distilled water.

ART. II.

The different appearances of the urine best distinguished, by attending only to the changes that take place in the colourless gross matter.

IX.

1. Conceiving, from the theory of fevers in general, that the presence of the paroxysms and remissions of these fevers, would always be discovered by the state of the febrile spasmodic stricture; that the state of the febrile spasmodic stricture

would be ascertained by the state of the urine's thickness or consistence ; and, ultimately, that the state of the urine's consistence would be ascertained by the state of the *gross matter* ; I was led, from this chain of anticipation, to keep my attention fixed upon *the gross matter alone abstracted from colour and every other quality*. By this means, I discovered *the following distinctions or characters in the urine, arising from the different forms in which the gross matter, thus ideally abstracted, made its appearance at different stages of the disease*.

1. *Mucose urine*, which is urine exhibiting equally, thro' the whole of its substance, some degree of mucous, or viscid appearance, without any mixture of cloud or powder.

2. *Nubiferous urine*, which is urine exhibiting one or more mucus-like clouds suspended in it, and is of two kinds, or rather degrees, viz.

a. *Nubilated urine*, so called, when the cloud is single, and considerably large and compact.

b. *Nubeculated urine*, so called when, instead of a single, compact and considerable cloud, there appear thin light nubeculae.

3. *Nubipulviferous urine*, which is urine exhibiting a mixture of cloudy and powdery matter.

4. *Pulviferous urine*, which is urine exhibiting a powdery matter suspended in it, which at first gives it a turbid appearance, and soon falls to the bottom, in the form of a sediment.

ART. III.

Certain appearances in the state of the gross matter of the urine, constantly connected with certain periods and revolutions in fevers.

X.

1. These distinctions, or characters arising from the state

of the gross matter being ascertained, I at length discovered that certain characters were constantly connected with certain periods and revolutions of the disease, and in the following manner.

In the first lunar period.

That, during the nocturno-meridional paroxysms, the urine constantly became *mucose* || ; and continued so during this period.

That, during this period, the urine of the nocturno-meridional paroxysms was constantly *less mucose*, than the urine of their respective inter-meridional remissions.

In the first interlunar interval.

That, upon passing from the first lunar period into the first interlunar interval, the urine, from being only *mucose*, generally became *nubilated* § ; and continued so during the interval.

That during this interval, the urine of the nocturno-meridional paroxysms was constantly *less nubilated* than the urine of their respective inter-meridional remissions.

In the second lunar period.

That, upon passing from the first interlunar interval into the second lunar period, the urine, from being *much nubilated*, became *nubilated in a less degree* *, and sometimes *nubeculated* ; and continued so during this period.

|| Sometimes, when the fever is high, the urine will become perfectly *limpid* in the paroxysms of this period ; and, on the other hand, when the fever is moderate, the urine will exhibit the appearance of a cloud : But these exceptions are to be considered as deviations only from the general tendency which prevails at this period.

§ In order to see this change distinctly, it is necessary to watch the urine of the inter-meridional remissions, especially that of the morning, on the 4th, 5th, and even the 6th day after the day of the change and full of the moon ; but the urine seldom fails to shew some appearance of cloud in the course of this interval, however late it may appear.

* I have seen the urine in the paroxysms of this period losing its cloud, become again *mucose*, and even *limpid*. But these are deviations from the general rule, and do not happen when the fever is well treated from the beginning.

That, during this period, the urine of the nocturno-meridional paroxysms was constantly *less nubilated* or *nubeculated*, than the urine of their respective inter-meridional remissions.

In the second interlunar interval.

That, upon passing from the second lunar period into the second interlunar interval, the urine, from being nubilated or nubeculated, generally became *nubipulviferous* or *pulviferous*, and continued so during part †, or the whole of this interval.

That, during this interval, the urine of the nocturno-meridional paroxysms, so long as these were observable, was constantly *less nubipulviferous* or *pulviferous* than the urine of their respective inter-meridional remissions.

In the lunar periods, and interlunar intervals, in fevers continuing beyond the second interlunar interval.

In fevers running on through more distant lunar periods and interlunar intervals, the urine of the second lunar interval, in such cases, is not always uniform or constant in its appearance. Sometimes it becomes only *nubilated* or *nubeculated* in a greater degree, than it was in the preceding lunar period; and sometimes the *nubiferous* and *pulviferous* state appearing to be united, a sediment partaking of both is deposited at the bottom*.

As the fever advances through lunar periods and interlunar intervals still more remote, if the fever has been properly treated, the grosser mucus and powdery particles, which gave

† Although the slight nocturno-meridional paroxysms should continue for some time during this interval, it will be remembered, that the remarkable change which takes place at the commencement of it, is considered, (when the fever terminates in it,) as the *final crisis* of such forms; which of course constitute forms of 21 days. This matter is fully explained in our Treatise on Putrid Intestinal Remitting Fevers, page 89.

* The *nubipulviferous* state of the urine is a common thing in all the *intermediate* or *imperfect* crises, as well as in those which prove *final* or *perfect*; and it is not easy to predict from this circumstance alone whether it is to prove an *intermediate* or a *final* crisis.

the urine these appearances, being at length carried off or exhausted, a greater or less proportion of thin light *nubeculae*, floating in different places ; or of a *fine powdery deposition*, are now the only circumstances in the state of its consistence that distinguish the lunar periods from the interlunar intervals, and the meridional paroxysms from the inter-meridional remissions.

XI.

1. The *diurno-meridional paroxysms, and their respective evening inter-meridional remissions*, we have observed in our treatise ||, are obvious only at certain stages of the disease ; but, so far as we have been able to discern them, the appearances of the urine in these paroxysms and remissions are perfectly analogous to those of the nocturno-meridional paroxysms, and their respective morning inter-meridional remissions. If, disappearing or becoming obscure at certain times, they do not, like them, exhibit so continued a chain of coincidences to prove the connection of paroxysms with meridional periods, and of remissions with inter-meridional intervals, from the beginning to the end of fevers ; they, however concur, when they do appear, to strengthen the evidence afforded by the urine in the nocturno-meridional paroxysms, and their subsequent remissions : and shew that the action of sol-lunar influence is uniform and constant, and, in similar circumstances, displays a tendency to exert itself in a similar manner, though not exactly with equal effect ; the cause of which deviation may perhaps be discovered hereafter.

XII.

1. It appears, by the method which we have adopted for examining the urine, that all the different changes which have been remarked in its consistence may be referred to various proportions of mucous matter or powdery particles ; all which may be comprehended under the general term of *gross matter*, and thus distinguished from the particles which give the urine its colour : which may therefore be nominated *colouring matter*.

2. Although the *colouring matter*, at the beginning of fevers, always becomes *red*, and generally continues to be more or less of this hue, so long as the paroxysms return with any force; yet, if the bowels have been kept sufficiently open, or if bark has been given in any considerable quantity, it will again approach to its natural *amber* or *yellow* colour, and remain in this state for many days previous to the final termination of the disease.

3. But, with respect to the *colouring matter*, we have now to observe, that, whether it be yellow or red, the changes which it undergoes in the meridional paroxysms and inter-meridional remissions, are in degree analogous, and in time coincident with those which take place in the matter which we have denominated gross. That is to say, that, as the gross matter, whether mucosc, eloudy, or powdery, appears throughout the disease in *less quantity* during the meridional paroxysms than the inter-meridional remissions, in like manner does the urine at these times become *less high* in its colour than in the remissions; or, in other words, contains the colouring matter in a *smaller proportion*. And thus it appears that the fluctuation which takes place in the state of the gross matter, and in that of the colouring matter, *both unite to demonstrate the prevailing tendency of the urine to be more thin during the paroxysms than it is in the remissions*.

4. Remarkable changes also in the *colouring matter*, analogous in degree and coincident in time, with those observed in the gross matter, constantly happen on the first, second, or third day, after the transition from the lunar periods into the inter-lunar intervals, and on the first, second, or third day after the transition from the interlunar interval into the lunar periods; and mutually concur to demonstrate, that it is likewise the prevailing tendency of the urine to be more thin during the lunar periods than it is in interlunar intervals; but in detecting the changes of the febrile state it will always be found best to watch the appearances of the gross matter.

ART. IV.

The different appearances of the gross matter of the urine occurring in a fever of 21 days commeneing with the lunar period, exhibited as an example to illustrate the appearances and changes of the urine in all other forms.

XIII.

1. Having obtained this knowledge, (IX. X. XI. XII.) I found myself possessed of a criterion far superior to that which arose from the contemplation of the *other symptoms*. *These appearances in the urine*, like astronomical observations taken at sea, which correct and adjust the common reckoning of the ship's course, concurring uniformly and proportionately with corresponding degrees of intension and remission in the febrile symptoms, established at once *the presence or absence of the paroxysms at particular times*, upon the testimony of ocular demonstration; and enabled me to decide with confidence respecting *the coincidences of the febrile paroxysms and remissions with the meridional periods and inter meridional intervals: and also respecting the coincidences of the septenary increase and decrease of the fever with the lunar periods and interlunar intervals:* on the reality of which the whole of this doctrine ultimately depends.

2. It was therefore my original intention *to make my observations on the urine, the foundation of my induction respecting the laws of sol-lunar influence*. But, finding myself under the necessity of publishing my Treatise on Putrid Intestinal Remitting Fevers when I was unequal to the task of giving them that degree of perspicuity which they required, I thought it better to confine myself to general expressions respecting the grounds of my reasoning, than run the risk of becoming confused or obscure by an arrangement not perfectly precise or clear.

3. Such being the use and importance of examining the urine, I have endeavoured, in the following TABLE, to exhibit an idea of the ordinary changes which it undergoes in

one of the most common forms of fevers, viz. a form of 21 days, commencing with a lunar period; where laxatives were chiefly employed for the cure, and little or no bark given to alter its natural appearance §: hoping that this single example will serve as a clue by which future inquirers may be directed in carrying on their observations through all the various forms of these fevers; *whether they belong to the class of those which make their first attack in the lunar periods: or of those which make their first attack in the interlunar intervals.*

§ I think the evident effect of the bark, when given at the rate of 6 or 8 drams a day, is soon to change the urine to a yellow or at least a lighter colour; but, unless it stop the paroxysms, although it may diminish, it does not prevent, the daily or septenary fluctuations in the state of the gross matter.

EXPLANATION OF THE OPPOSITE PAGE.

(? ? ?) Although the symptoms of the evening inter-meridional remissions become obscure towards the 4th day of the lunar periods, it is possible that an evident tendency to remit at this time may be discovered by some appearances in the urine, if examined with sufficient attention. The observations which I have made myself being imperfect, I have left the character of the urine in the evening inter-meridional intervals of the 5th, 6th, and 7th day, to be determined hereafter; expressing my uncertainty by points of interrogation.

N.B. d. m. p. denotes diurno-meridional paroxysm.

e. i. r. ——— evening inter-meridional remission.

n. m. p. ——— nocturno-meridional paroxysm.

m. i. r. ——— morning inter-meridional remission.

The appearances of the urine in a fever of 21 days.

In the 1st lunar period	1 day	$\left\{ \begin{array}{ll} \text{d. m. p.} & \text{—mucose.} \\ \text{e. i. r.} & \text{more.} \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	1 day	of the fever
	2 day	$\left\{ \begin{array}{ll} \text{d. m. p.} & \text{less.} \\ \text{e. i. r.} & \text{more.} \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	2 day	
	3 day	$\left\{ \begin{array}{ll} \text{d. m. p.} & \text{less.} \\ \text{e. i. r.} & \text{more.} \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	3 day	
	4 day	$\left\{ \begin{array}{ll} \text{d. m. p.} & \text{less.} \\ \text{e. i. r.} & \text{more.} \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	4 day	
	5 day	$\left\{ \begin{array}{ll} \text{d. m. p.} & \text{less.} \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	5 day	
	6 day	$\left\{ \begin{array}{ll} \text{d. m. p.} & \text{less.} \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	6 day	
	7 day	$\left\{ \begin{array}{ll} \text{d. m. p.} & \text{less.} \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{—nubilated.} \end{array} \right.$	7 day	

EXPLANATION OF THE OPPOSITE PAGE.

(? ? ? ?). In our Treatise on Putrid Intestinal Remitting Fevers, page 82, we have said that the meridional paroxysms, after having gone through the first lunar period, become, after the commencement of the interval, much more slight and obscure, and generally confined to the nocturnal period ; and therefore, to express this tendency in the *Diagram* of that Treatise, we have made use of *lighter oval figures*. To express it in this table, we have put points of interrogation, or uncertainty in the place of the diurno-meridional paroxysms, and also of their remissions ; and have inserted nocturno-meridional paroxysms only, and their respective morning inter-meridional remissions, to the urine of which my observations are chiefly confined.

(p p p.) With respect to the nocturno-meridional paroxysms of this interval, I must farther observe, that the tendency of every succeeding paroxysm is to return at a later hour than the former ; and towards the end of the interval, I have observed head-achs and other symptoms of the paroxysm defer their attack till five and six in the morning, and continue during the remaining part of the morning inter-meridional interval. But my observations at this time are imperfect ; and the rule of such deviations must be ascertained hereafter. To express our uncertainty upon this point, we have in this table distinguished the nocturno-meridional paroxysms of the 5th, 6th, and 7th day, by the letter *p.* placed after each, to denote their tendency to *postpone*, and to appear towards morning.

The appearances of the urine in a fever of 21 days.

In the 1st interlunar interval	1 day	$\left\{ \begin{array}{lll} \text{d. m. p.} & ?\text{—nubilated.} \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	8 day	} of the fever.
	2 day	$\left\{ \begin{array}{lll} \text{d. m. p.} & ? \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	9 day	
	3 day	$\left\{ \begin{array}{lll} \text{d. m. p.} & ? \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	10 day	
	4 day	$\left\{ \begin{array}{lll} \text{d. m. p.} & ? \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	11 day	
	5 day	$\left\{ \begin{array}{lll} \text{d. m. p.} & ? \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less. p.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	12 day	
	6 day	$\left\{ \begin{array}{lll} \text{d. m. p.} & ? \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less. p.} \\ \text{m. i. r.} & \text{more.} \end{array} \right.$	13 day	
	7 day	$\left\{ \begin{array}{lll} \text{d. m. p.} & ? \\ \text{e. i. r.} & ? \\ \text{n. m. p.} & \text{less. p.} \\ \text{m. i. r.} & \text{—nubilated or nubecu-} \end{array} \right.$	14 day	

EXPLANATION OF THE OPPOSITE PAGE.

(1.) In our Treatise on Putrid Intestinal Remitting Fevers, page 82, we have observed that the meridional paroxysms having entered into a 2d lunar period, suffer an exacerbation nearly similar to that which they have suffered in the 1st. This exacerbation, however, does not always shew itself in the first day of this period, either in the symptoms, or urine; neither must we expect to find their evening inter-meridional remissions at the beginning of this period so distinctly marked as those in the first lunar period. Their obscurity, toward the end is represented, as it is in the first lunar period, by leaving the character of the urine in the evening inter-meridional intervals of the 5th, 6th, and 7th day, to be filled up hereafter, and by expressing it, in the mean time, by points of interrogation or doubt.

From the inseparable connection which the attack and termination of the paroxysms of fevers have with *two different points of time, within the space of 24 hours, viz. the diurno and nocturno-meridional periods and intervals*, it will now appear obvious, as suggested before in paragraph (XXI.), that the duration of fevers cannot be measured exactly by complete days; a circumstance well known to Hippocrates; but never till now explained. *Vid. Praenotiones, Sect. XX.*

The appearances of the urine in a fever of 21 days.

In the 2d lunar period	1 day	{	d. m. p. —nubilated or nubecu- e. i. r. more. [lated.] n. m. p. less. m. i. r. more.	}	15 day
	2 day	{	d. m. p. less. e. i. r. more. n. m. p. less. m. i. r. more.	}	16 day
	3 day	{	d. m. p. less. e. i. r. more. n. m. p. less. m. i. r. more.	}	17 day
	4 day	{	d. m. p. less. e. i. r. more. n. m. p. less. m. i. r. more.	}	18 day
	5 day	{	d. m. p. less. e. i. r. ? n. m. p. less. m. i. r. more.	}	19 day
	6 day	{	d. m. p. less. e. i. r. ? n. m. p. less. m. i. r. more.	}	20 day
	7 day	{	d. m. p. less. e. i. r. ? n. m. p. less. [pulviferous. m. i. r. —nubipulviferous or	}	21 day

of the fever.

*

In the course of the morning inter-meridional remission which follows the nocturno-meridional paroxysm of the 21st day, and those that follow in the 2d interlunar interval, we have reason to expect a *pulviferous* or *nubipulviferous* deposition in the urine ; and, generally, a *perfect crisis* *.

ART. V.

The appearances of the urine in the form of 21 days, will serve to illustrate its tendency in all the forms which make their first attacks in the lunar periods.

XIV.

1. These variations which we have described (IX. X.) in the consistence of the urine, are generally sufficiently obvious to a careful observer, continuing his attention, at certain hours of the day and night, from the beginning of the disease to its termination ; and the tendency of the urine in the form of 21 days, which we have given as an example, with some allowances, will serve to illustrate the changes which it undergoes in all fevers making their first attack in the lunar period, *whether they terminate in the 1st interlunar interval ; in the 2d, like the example ; or in interlunar intervals still more remote.*

a. *Respecting forms that terminate in the 1st interlunar interval,* the character of the urine, in the 1st lunar period, being, like that of the example, generally *mucose* and not *nubiferous* or *pulviferous*, indicates a tendency to be *thin*, similar to that of the example ; and the paroxysms and remissions of this period are also distinguished by a similar fluctuation in the state of its general character.

* We have learnt however, since this treatise was first published, that, if the greater parallaxes of the moon prevail at this juncture, the crises and changes of the urine with which it is otherwise generally accompanied, may be deferred until the parallaxes have fallen considerably from their greatest height. (new.)

Upon the commencement of the first interlunar interval, the urine in these forms (a) undergoes a change similar also to that in the example, becoming then *nubiferous* and, when the fever is about to terminate, generally *nubipulviferous* or *pulviferous*—thus indicating at this time a similar tendency to be *thick*. The urine of the paroxysms and remissions, as long as they continue to return, is constantly distinguished by a fluctuation similar to that in the example, in the state of the general character, whether it be *nubiferous*, *nubipulviferous*, or *pulviferous*.

b. *Respecting all forms beginning like the example in the lunar period, and terminating in the second interlunar interval*, of course the changes and appearances of the urine are nearly coincident, analogous, and similar to it.

c. *Respecting the forms of fevers running on to terminate in interlunar intervals beyond the second*, I have shortly to observe, that, in the lunar periods, and interlunar intervals, through which they pass, *previous to the commencement of the 3d lunar period*, a tendency to be *thin* during the former, and *thick* during the latter, similar to that in the example, is to be discerned in the urine; that similar changes are to be observed in passing from the one to the other; and that a similar fluctuation in that state of its consistence which forms its general character in different lunar periods, and interlunar intervals, uniformly distinguishes the meridional paroxysms and remissions of each. But, *after the commencement of the 3d lunar period*, as the symptoms of the meridional paroxysms in such cases generally become, if the disease has been well managed, much less violent, and the lunar periods, of course, less strongly distinguished, so have we to observe respecting the urine, that its general character, in different lunar periods and interlunar intervals, the changes which these general characters undergo in passing from the one to the other, and the fluctuation which takes place in the state of these general characters,

in the meridional paroxysms and corresponding remissions, all in like manner become less conspicuous and distinct than before. And, as it often happens in the later stages of fevers protracted beyond the commencement of the 3d lunar period, that the symptoms of exacerbation, which distinguish the general character of the lunar periods, do not shew themselves before the 4th, 5th, 6th, and even 7th day of these periods, so the urine corresponding always with the state of the fever, often manifests no obvious change in its appearance, until one or other of these latter days which I have specified.

ART. VI.

The appearances of the urine in the form of 21 days, will serve also to illustrate its tendency in all fevers which make their first attack in the interlunar intervals.

XV.

1. With respect to fevers making their first attacks in the interlunar intervals, as there is one remarkable difference to be observed between them and fevers making their first attacks in the lunar periods in the state and progression of their symptoms, so there is, in like manner, a remarkable difference between them in the appearance of the urine. In fevers attacking in the lunar periods, the symptoms become quickly more and more violent every day, and generally get to their greatest height before the end of the period in which they attack; and the urine presently acquires the character of being less muddy, or more clear and thin than in its natural state. In fevers attacking in the interlunar intervals, the symptoms, during the interval in which they attack, generally shew little inclination to become violent, and the urine seldom acquires that degree of clearness or thinness which marks the character of the urine in the beginning of fevers making their first attack in the lunar periods.

2. When fevers attacking in the interlunar intervals enter

the first lunar period, the symptoms and urine then assume the character peculiar to fevers making their first attack in the lunar period, and then becoming, as it were, coincident and analogous to them, they proceed in a similar manner thro' the rest of their course. And, having remarked the difference to be observed in the urine of these two distinctions of fevers during their first stage (1), we have only to add, that due regard being paid to that exception, *the urine in the form of 21 days may be assumed to illustrate the state and variation of the urine in all forms making their first attack in the interlunar intervals ; as well as in those which make their first attack in the lunar periods.*

CHAPTER IV.

INFERENCES DRAWN FROM THE ANALYSIS.

SECTION I.

Respecting the action of sol-lunar influence on the condition of the spasmodic state in putrid intestinal remitting fevers.

XVI.

1. IN collecting the sum of these observations, it appears that the urine of different meridional paroxysms thro' which fevers pass, is constantly distinguished by some peculiar appearance, indicating *a greater degree of thinness* than in the inter-meridional remissions; and farther, that the urine of meridional paroxysms happening in the lunar periods, is constantly distinguished by some peculiar appearance, indicating *a greater degree of thinness* than in the urine of meridional paroxysms happening in the interlunar intervals: so that there is to be observed in the state of the urine a *fluctuation* coincident with that of the ocean, respecting the times of the alternate changes which both undergo daily; and also those which they undergo in the lunar periods and interlunar intervals, in a manner so remarkable and striking, that no man who is capable of making a proper distinction between the nature of induction and fiction, can possibly incline to disjoin them, or refer them to different causes.

But it is generally allowed, that *the peculiar thinness of the urine*, which distinguishes the paroxysms of fevers, is produced by a *spasmodic affection*, or diseased constriction of the small vessels through which it passes, connected with a similar affection extending itself, at the same time, to other parts; and probably, in some degree over the whole vascular system. We therefore infer from these premises the following theorem.

THEOREM I.

“ That the peculiar thinness of the urine in the paroxysms
 “ of putrid intestinal remitting fevers, is owing to a spas-
 “ modic affection or diseased constriction of the vascular
 “ system ; that the fluctuation which we have described
 “ in the state of its thinness, at certain periods and inter-
 “ vals of the day and month, is owing to a corresponding
 “ fluctuation in the state of that spasmodic affection ; and
 “ ultimately, that the fluctuation which takes place in the
 “ state of that spasmodic affection, cannot reasonably be
 “ referred to any other cause than a corresponding fluctuation
 “ in the state of that influence which produces
 “ the ebbing and flowing of the sea, with which it constantly
 “ coincides in point of time.”

SECTION II.

Respecting the action of sol-lunar influence on the condition of the febrile spasmodic state in all other fevers.

XVII.

1. Sensible of the fallacy of reasoning from *analogy*, and of the great abuse that has been made of it in medicine, I have carefully abstained from drawing a single argument from phaenomena, which I did not observe myself in fevers decidedly putrid, intestinal, remitting, and infectious. Bilious remitting and intermitting fevers, where infection was not so evident or certain, (especially those connected with a diseased liver, which I have seen beyond number returning regularly at the lunar periods for several months, and in many cases for years together,) afforded evidence of sol-lunar influence still more striking, than the fevers of which I have treated. But the analogy being questionable, I was unwilling to expose a doctrine of such importance to any objection of this kind, by employing it.

Analogy, however, is by no means to be rejected altogether; and, from what I have established respecting putrid intestinal

remitting fevers, from what I have seen in bilious, and all other kinds of fevers *, and from what Dr Mead, Dr Lind, Dr Jackson, and the different authors to whom they refer, have observed on this subject †, I think there is much reason to admit the probability of the following proposition upon this ground.

THEOREM II.

“ That sol-lunar influence regulates the condition of the febrile
 “ spasmodic state, or diseased constriction of the vascular
 “ system, in every disorder without exception, where the
 “ degree of periodical febrile paroxysm exists, in the same
 “ manner that it does in fevers that are putrid, intestinal,
 “ and remitting.”

SECTION III.

Respecting the action of sol-lunar influence on the condition of the spasmodic or constrictive state of the vascular system in other diseases, and in health.

XVIII.

1. Proceeding on the analogical conclusion we have admitted respecting fevers ; and on various observations made in other diseases § ; and in the case of certain derangements and

* Vide B. at the end of this Treatise.

† Vide Dr Jackson's Treatise on the Connection of the New and Full Moon with the Invasion and Relapse of Fevers, in the London Medical Journal for 1787 ; and also his Treatise on the Fevers of Jamaica in 1791 ; Dr Mead's Treatise on the Influence of the Sun and Moon, &c. Dr Lind's Thesis, &c. ; and my 1st Treatise published at Calcutta in 1784.

§ My observations on sol-lunar influence in India have not been confined to fevers. Epilepsies, paralytic affections, head-achs, inflammations of the eyes *a*, asthmas, phthisical coughs, gouts, rheumatisms, tooth-achs, pains and swelling of the liver and spleen, diarrhoeas, cholics, costiveness, complaints in the urinary passages, boils *b*, ulcers, sores, and eruptions of various kinds, very often assumed an intermittent form, and returned regularly with the full and change of the moon, and disappeared or diminished with the intervals.

a Vid. C. at the end of this Treatise. *b* Vid. D. at the end of this Treatise.

revolutions || in weak and delicate constitutions, I will venture to advance another proposition, the truth of which I presume will also be confirmed by future observations.

THEOREM III.

“ That there is much reason to believe that sol-lunar influence
 “ regulates the condition of the spasmodic or constrictive
 “ state of the vascular system, in all other diseases ; and
 “ affects it even in health.”

SECTION IV.

Respecting the sufficiency of the spasmodic or constrictive state of the vascular system to produce the paroxysms and changes in fever, and other diseases ; and certain derangements and revolutions in health.

XIX.

1. In the preceding propositions we have been led to infer, that *sol-lunar influence regulates the condition of the spasmodic or constrictive power of the vascular system, in diseases and in health.* We are now to infer, that the condition of the constrictive power of the vascular system, and its action, is a circumstance upon which the state of disease and health in a great measure depends ; and that different variations in the degree, and distribution of this constrictive power, with other circumstances that must be included and considered, are *sufficient* for explaining all the periodical revolutions which take place in these two states. It is not, however, my intention, at this time, to enter into a particular proof of this inference. We know that wonderful effects may be produced by a *general*

|| Under this head I consider the fits of teething in children, attacks of the hæmorrhoids, child-bearing, and those revolutions which take place monthly in the constitution of females. With respect to the three former, I have constantly observed them very much connected with the change and full of the moon ; and respecting the last, although I have taken no pains to ascertain it, yet I have met with many occurrences which induce me to refer it to the same cause.

relaxation or constriction of the whole vascular system. We also know what changes may be effected by *a relaxation or constriction confined to a particular organ or part.* From these two considerations, we can easily conceive, and safely infer, the extensive influence of the constrictive power of the vascular system; and on this ground we propose the following proposition.

THEOREM IV.

“ That the condition or state of the constrictive power and action of the vascular system, is a circumstance upon which the state of disease and health in a great measure depends; and that different variations in the degree and distribution of this constrictive power, with other circumstances to be included and considered, are capable of explaining all the periodical revolutions which take place in health and disease.”

2. In what manner, or by what means, sol-lunar influence acts upon the condition of the constrictive power of the vascular system, we do not at present pretend to say: But we shall not despair of seeing a rational solution of this question, upon principles connected with those which have been demonstrated by the immortal Newton.

SECTION V.

Respecting the Vis Medicatrix Naturae.

XX.

1. Collecting from these propositions that there is reason to believe that sol-lunar influence presides over the condition of the spasmodic or constrictive state of the vascular system in fevers, all other diseases, and in health; and in fevers, all other diseases, and in health, produces these remarkable efforts and changes which hitherto have been referred by physicians to a power existing in the constitution itself, and acting

independently of any external agent, the *vis medicatrix naturae*, we obtain a new proposition in medicine of singular extent and importance, viz.

THEOREM V.

“ That there is reason to believe that the celebrated *VIS MEDICATRIX NATURAE*, the producer of paroxysms, the giver of remissions, the deity of some physicians, the devil of others, is nothing more than sol-lunar influence exerting itself upon the condition of the constrictive power of the vascular system, in disease and in health, according to laws that are uniform and universal.”

2. Having shewn that there is reason to believe the *existence* and *sufficiency* of a natural cause, to account for all the periodical changes of the human frame, the first rule* of philosophy forbids us to admit or suppose another: with respect to this ideal divinity, the *vis medicatrix*, we therefore pronounce, in the words of the poet,

“ *Nec Deus intersit, &c. &c.*”

SECTION VI.

Respecting the extent of sol-lunar influence over the universe.

XXI.

1. In the course of this investigation, it appears to be probable, that the dominion of sol-lunar influence over the human race, is uniform and universal.

In the diseases of horses and dogs in India, I have on many occasions observed the meridional periods marked by extraordinary paroxysms; and, in the lunar periods, I have seen these extraordinary paroxysms distinguished by an uncommon degree of exacerbation.

* Vide Newton's Principia Reg. Philosoph. I.

That the constitution of certain birds and fishes undergoes a regular change corresponding with the revolutions of the moon, is a fact established by the observation of every age, in different quarters of the globe.

2. With respect to vegetables, it has been observed by Dr Mead, that husbandmen are regulated by the moon in planting and managing trees; and extraordinary effects are ascribed by farmers to the full moons of harvest, which cannot proceed from any increase of heat communicated by the lunar rays. But I do not know that any direct experiments have been made, by which we might infer sol-lunar influence in the vegetation of plants, independent of the effects that may be produced by its action on the state of the atmosphere which surrounds them.

3. With regard to the effects of sol-lunar influence on the state of the atmosphere, although observations concerning them have not been reduced to any regularity or system, they seem, however, to be universally admitted. Neither have my own observations been sufficiently particular or precise. But I can declare, in general, that, in India, the meridional periods, both diurnal and nocturnal, were distinguished by remarkable changes or paroxysms in the state of the weather; and that these paroxysms were most remarkable at the lunar periods.

4. The connexion of the ebbing and flowing of the sea with sol-lunar influence, is acknowledged by all mankind, wherever there is an opportunity of observing it.

5. Taking a comprehensive view of this subject, I think there is much reason to believe, that all the different kingdoms of nature, at least the animal, vegetable, and atmospheric and aqueous departments, are under the dominion of sol-lunar influence; and that, like the animal constitution, they are subject to daily and monthly revolutions.

SOL-LUNAR INFLUENCE.

Finally, “ if philosophers, in all the departments of nature,
“ could be prevailed upon to take the trouble of distinguish-
“ ing and arranging the phaenomena of the different hours of
“ the day, and of the different days of the month, according
“ to the divisions of time which are suggested by their coin-
“ cidence, with particular relative positions of the sun and
“ moon,” I think it is possible that the phaenomena collected
under the different divisions of this arrangement, might pre-
sent different collective or general characters, which did not
appear before ; and that these characters discovering one com-
mon corresponding tendency in all, might at last enable us to
infer upon the foundation of a perfect induction.

THEOREM VI.

“ That sol-lunar influence exerts its dominion, in some degree
“ over every production ; and in every operation and re-
“ volution of nature.”

NOTES.

A.

Hints respecting Bilious Fevers.

THERE occur very frequently in India, fevers which seem to arise, *without any infection*, from the absorption of bile accumulated in the intestines. This increased secretion and accumulation is almost always connected with a *diseased liver*. Such fevers will often run on for weeks together, with daily paroxysms and lunar exacerbations, similar to those of infectious fevers, and it is often very difficult to distinguish them.

The *urine* of fevers *purely bilious* is always deeply tinged with bile; and changes similar to those in fevers that are infectious, are to be observed in the state of the colouring and colourless matter at particular times of the day and month; and concur to demonstrate the operation of sol-lunar influence, and the importance of attending to its laws in directing the cure.

Bilious fevers are now found to be much more frequent in northern latitudes than they were conceived to be some years ago; and I presume that it will soon be discovered, “that the
“ whole tribe of diseases hitherto denominated hypochondriac, and so little understood, are actually nothing more than a
“ certain concurrence of febrile symptoms connected with a
“ diseased state of the liver, and to be treated in a manner similar to that which is followed by the surgeons of this country*, who have practised in warm climates.” The hypo-

* India.

chondriac disease in India is a diseased liver ; and gouts and rheumatisms are febrile states produced by the absorption of putrid and bilious matter from the intestines.

B.

Hints respecting the Small Pox.

Since my Treatise published at Calcutta in 1784, I have had an opportunity of adding very few observations to those which I had made at that time respecting the small pox ; but all of them induced me to infer, that this disease is greatly affected by sol-lunar influence ; and I am inclined to think that it will be found, “ that the coincidence of the eruptive fever with
“ the lunar periods, increases the severity of the fever, and
“ renders the pustles more numerous :” on the other hand,
“ that the coincidence of the eruptive fever with the interlu-
“ nar intervals, tends to moderate the fever, and to render
“ the pustles less numerous : and that there is reason to sup-
“ pose, that the great diversity which appears in different cas-
“ es of small pox, may be owing, in a great measure, to the
“ communication of the infection, and the attack of the erup-
“ tive fever at particular stages of the lunar periods and in-
“ terlunar intervals.”

I have made a few experiments and observations in the case of patients who were inoculated, which have led me to those conclusions, and I have reason to suspect, from several instances, “ that the secondary fever of the small pox, caught in the
“ natural way, which has been so fatal to mankind, is occa-
“ sioned by the coincidence of the lunar periods with a cer-
“ tain state or stage of the disease ; and does not depend, as
“ it has always been supposed, upon any particular change
“ connected with a particular day of its duration.”

C.

Respecting the Cure of certain Species of Ophthalmia.

At Calcutta, Benares, and Lucknow, I have met with a species of ophthalmia attacking about the coming on of the rains in June, the proper method of treating which is probably not generally known ; that is to say, that bleeding, purging, blis-

tering, fomenting, and applying the common colyria of Europe, have little effect in abating the symptoms, or preventing the disease from proceeding to the greatest height. This formidable disorder comes on gradually with a slight swelling in the eye-lids and inflammation in the eye, and from the very first almost they begin to discharge an humour which thickens and glues the eyes, and renders them exceedingly uneasy. Besides this, the patient is constantly teized with the sensation of a particle of sand, or some hard substance, between the eye ball and the upper eye-lid. In the space of 4 or 5 days, the inflammation of the eye has increased, in spite of all common remedies, to so great a pitch, that it is unable to endure the smallest degree of light; the pain now begins to shoot through the head, and to produce a terrible head-ach, and the sclerotic coat swells and rises above the surface of the cornea. " At night, all the symptoms are aggravated, but, towards morning, they begin to abate, and continue more moderate till the time of their exacerbation at night; and relapses and " exacerbations of the disease are obvious at the lunar periods."

To cure this disorder, the bowels must be cleared by a dose of calomel at bed time, succeeded by a glass of the cathartic salts in the morning; and the Peruvian bark in substance is then to be given in as great a quantity as the stomach will bear it, until the nocturnal exacerbation is entirely stopt, or returns with little violence.

The bark puts a stop to the paroxysms of pain with great certainty; but, to unload the eye in the mean time, and to procure more *instantaneous* relief, I am now to recommend the application of an ointment which was first communicated to me by a native of India.

R.

" Take one table spoonful and a half of fresh butter, keep it boiling in a glazed pot for half an hour, free it of all impurities, and then carefully mix with it in a mortar one scruple of opium and one dram of alum, previously rubbed together into an impalpable powder."

N. B. " The original recipé contained a small proportion of
 " turmerick and nim-leaf, but not being acquainted with
 " the effects of these articles, I was afraid, and left them
 " out."

A little of this ointment must be insinuated within the eyelids all along their edges. At first, it occasions a great degree of smarting pain, which continues about a minute, after which it begins to abate, and then a thin watery discharge runs copiously down the cheeks. This operation, I think, succeeds best during the remission of the paroxysm, and produces then the greatest discharge and relief. Before I made use of it, the disease had always advanced four or five days. If it were used at the beginning, I think it is probable that it might prevent it from going to a great height.

After the bark has stopt the nocturnal exacerbation of pain, and the eyes have been unloaded by two or three applications of this ointment, preparations of lead, in the form of a lotion during the day, and of an ointment during the night, complete the cure ; which, however, is often very tedious if the disease has been severe †.

D.

Respecting the cure of the Furunculus, or Boil, which terminates by the separation of a tough Core.

These boils are always attended with some degree of fever, the paroxysms and exacerbations of which are affected by the influence of the moon. They appear, I think, most frequently in the months of May and June, and are exceedingly painful and troublesome.

So long as the tough core remains attached to the inside of the boil by its fibres, the pain and inflammation continue without remission. But, whenever these fibres are detached or

† This mode of treating ophthalmias does not seem to have been known to the surgeons who were employed in Egypt ; and I much lament, that, not being in India at the time that the expedition was ordered from Bengal, I had not an opportunity of recommending it to their attention. (*new.*)

destroyed by a suppuration all around it, the pain and inflammation then abate, and may almost instantly be entirely removed, by opening the skin, and evacuating the core and other matter.

The chief object, therefore, is to effect an immediate separation of the core from the interior surface of the boil by suppurative. But, in producing this desirable change, nature is, in most cases, exceedingly tedious; and the common applications used by Europeans give her little assistance.

About eight years ago, when afflicted with this complaint, I was advised by my own servants to try an application of the leaves of the *nim-tree*. The operation of this remedy was so sudden and conspicuous, that I have used it in a great number of cases which have occurred since that time with equal success. It will often produce, in the space of one or two days, what cannot be effected by common means in eight or ten; and I have therefore dedicated this note to make it more generally known.

The *nim-tree* is common every where all over the country from Calcutta to Lucknow. I believe it is common all over India, and therefore regret less my having neglected to obtain a botanical description of it.

R.

“ Take a handful of the leaves of the *nim-tree* fresh gathered, and having placed them on a fragment of the common
“ cudgerie or earthen pot, heat them quickly on the fire without drying them, or exhausting their moisture; in this state,
“ apply them to the boils in a heap, as hot as they can be endured; and having fastened them on, allow them to remain
“ for half an hour, when they must be relieved by a fresh application. This operation is generally performed by the
“ bearers who carry the palanken, and must be repeated till
“ the suppuration be completed.”

After the core is evacuated, the emplastr. diach. cum. gum draws the boil, and soon heals it.

TREATISE IV.

ON

SOL-LUNAR INFLUENCE

IN

FEVERS, &c.

This Treatise was originally published as the FIRST VOLUME of the intended System described in the Preface that is prefixed to it ; but having confined my views to the Publication of this Collection, it now appears as my FOURTH TREATISE.

TO
THE HONOURABLE
SIR JOHN SHORE, BARONET,
GOVERNOR-GENERAL OF BENGAL,
&c. &c. &c.

SIR,

IN permitting me to place under your protection this attempt to establish the Principles of a Theory, which is still new in Philosophy and Medicine, you have given it that countenance which it so much requires in its present state; and have conferred upon me an honour, and a lasting obligation.

I have the honour to be,

SIR,

Your most obedient and

Most humble servant,

FRANCIS BALFOUR.

TO

DOCTOR FRANCIS BALFOUR.

SIR,

THE Governor General in Council being always disposed to encourage Servants of the Company, in instances of Publications that promote science, or are calculated to do a general service, directs me to inform you, that the expence of your publication entitled, "*A Treatise on Sol-Lunar Influence in Fevers,*" shall be defrayed by Government.

You will therefore be pleased to circulate copies of this work to the different parts of the Country, where you think it will be useful; and likewise transmit 20 copies to this Office, to be forwarded to the Honourable the Court of Directors.

I am, SIR,

Your most obedient humble servant,

(Signed) C. SHAKESPEAR, *Sub-Secretary.*

Calcutta Council-Chamber, }
17th April 1794. }

P R E F A C E.

IF SOL-LUNAR INFLUENCE should ever be acknowledged as a law of nature, and universally received as a new principle, it may hereafter become a matter of curiosity, and even of instruction, to trace the steps by which it arose from obscurity to light.

The general idea of the system which I have endeavoured to unfold was first published at Calcutta in 1784, in a *Treatise on the Influence of the Moon in Fevers*, which was afterwards reprinted in England, and also inserted in Dr Duncan's Medical Commentaries *.

In 1790, in a *Treatise on Putrid Intestinal Remitting Fevers*, published at Edinburgh, the periodical return of febrile paroxysms, and their coincidence with the periodical intensions and remissions of sol-lunar power, which constitutes the foundation and proof of this theory, was investigated, described, and illustrated by two different plates exhibiting a synoptical view of the whole system. The *first part* of that treatise is a regular logical synthesis, arising from facts observed and collected by myself, to the discovery of certain prevailing tendencies in nature, and thence to axioms or general laws. The *second part* is an analysis, in which these axioms or laws are

* *Vid.* decade 1st. vol. IX. for 1783-4, where it is published at full length.

employed to explain some of the most remarkable phaenomena of fevers. The *third part* is an application of the principles of this theory to form general rules for our practice.

In 1791, in a *Treatise on the Action of Sol-lunar Influence*, &c. published at Edinburgh, I made an attempt to advance a step farther, by endeavouring to investigate the particular nature of that change which is produced in the state of the human body by the operation of this power; and in 1792, when about to return to Bengal, perceiving that my two last Treatises, the one explaining the *periods* of its action, the other the *mode*, or rather effect, formed a kind of system on this subject, they were on this account bound up together in one volume, under the more comprehensive title of a *Treatise on Sol-lunar Influence*.

The observations which Dr James Lind had made in Bengal in 1762, and published in his Thesis at Edinburgh in 1768, though few, could not fail to give weight to the opinions which were afterwards advanced in 1784, respecting the extensive dominion of sol-lunar influence in fevers; and Dr Jackson's observations on the connection of the new and full Moon with the invasion and relapses of fevers ‡, being more extensive and numerous, contributed still more to confirm that doctrine. But the support which our theory derived from these authorities was of short duration. In a paper of the London Medical Journal for 1787, Dr Lind retracts § his former opinion in

‡ *Vid.* London Medical Journal, vol. VIII. for 1787, part I.

§ In this paper Dr Lind ascribes the changes that are produced in the state of fevers at the full and change of the moon to the noxious vapours arising from

favour of sol-lunar influence, and refers the phaenomena which he had observed to another cause. And in a Treatise on the Fevers of Jamaica, published in 1791, Dr Jackson, adopting a new theory, has withdrawn, in a great degree, the countenance he gave it in his first publication.

The Reviewers and Commentators who have honoured my productions on this subject with their remarks, although all of them, excepting the Monthly Reviewers, seem to betray some symptoms of distrust, deserve my gratitude for the disposition they have shewn to treat me with politeness. And respecting some ingenious authors who have touched upon my opinions in their late publications, I find myself under the necessity of making the same observation, with similar acknowledgments.

It was natural to suppose that men of science in England, would be curious to be more fully informed respecting the foundation of a theory which pretended to the discovery of a new and active principle in medicine and philosophy. But a general silence and reserve prevailed every where on this subject; and left me no room to hesitate respecting the degree of credit it had obtained in my native country.

But notwithstanding these various causes of discouragement, I have never once wavered in my opinion. Every fresh examination of my system, every new observation has confirmed me more and more in my belief; and has enabled me to per-

the swamps occasioned by the high tides overflowing the low lands at these times; and he concludes that what is called lunar influence will no where be found, but where remitting and intermitting fevers are occasioned from muddy shores left by the ebbing of the tide.

ceive more distinctly, the extensive range of its application and use.

I must however confess, that the cold reception it has met with from the learned of Europe, and the profound silence with which it has been treated, for no less than nine years, by my brethren in Bengal, have often led me to apprehend, that it might pass away unnoticed; and be buried, after all, in oblivion. These apprehensions have again called my attention to a subject, which I had once abandoned for ever, and have roused me to the exertions I am now making.

In publishing this volume, however, I do not flatter myself that a doctrine, which proposes little less than a total reformation in the present theories and systems of medicine, with considerable innovations in other branches of philosophy, should be at once admitted into the schools of Europe, and embraced with rapidity. I content myself with indulging hopes similar to those expressed by Dr Cullen in his preface to his *Nosologia Methodica*, when inculcating the use of that study—"Ego quidem contentus ero, licet ipse nimirum parum
 "sim profectus, si ingenia plurima ad studii admodum ne-
 "glecti cultum statim excitavero. Ab aequalibus jam senes-
 "centibus vix aliquid hujusmodi expeeto; quorum alii studiis
 "nunquam dediti has Novitates, quas vocant, vel refugiant,
 "vel detrectare student; alii studiorum licet fautores, in qui-
 "bus etiam multum profecerint, tamen ut relicta repetant, et
 "quaedam forsitan dediscenda fateantur, vix adduci possunt.
 "Quoeirca hoc studium a junioribus fere promotum iri spe-
 "ro."

Such only are the hopes which I allow myself to indulge at present; and, in order to give all the encouragement and assistance I am able to those who may hereafter incline to prosecute these studies, I have undertaken to accomplish three different objects.

1st. “ To establish the reality of sol-lunar influence upon such evidence that it may no longer be a matter of doubt, but that future inquirers may proceed directly to investigate the particular laws and restrictions under which it acts upon the human frame.”

“ To this part of my subject I dedicate the whole of my FIRST VOLUME.”

2d. “ To investigate the particular laws and restrictions under which it acts, and to demonstrate the utility and importance of this knowledge, by applying it to explain the whole phaenomena of the paroxysms of fevers: namely, Their periodical septenary and daily attacks, and remissions; their various types of quotidian, tertian, &c. &c. their transition from one type to another; their occasional anticipations and postponings; their aggravations and abatements; their prolongations and contractions; their periodical relapses; their tendency to forms of certain duration, more frequently than to others; their crisis, perfect and imperfect; the coincident appearances of the urine; and the real nature of the *vis medicatrix naturae*; all of them investigations hitherto attempted by physicians in vain; and by Dr Cullen himself relinquished as researches in which he found no light to direct his way.”

“ In a SECOND VOLUME, I pledge myself to explain the
 “ whole of these phaenomena, in a manner that shall carry a-
 “ long with it a perfect conviction of its truth to every man
 “ of candour and sense, who shall take the trouble to exa-
 “ mine it.”

3d. “ To apply the knowledge we shall have thus obtain-
 “ ed to reform the theory, practice, and language of medi-
 “ cine; and to shew in what respects these may be still far-
 “ ther improved by applying it to prosecute new discoveries.
 “ This will furnish matter for a THIRD VOLUME.”

In having brought this, my FIRST VOLUME, to an end, I have performed the first part of my task; and in submitting it to the public in Bengal, where the whole of the facts on which my reasoning is founded have been observed, where the witnesses who testify these facts are all at hand, where it is in the power of every individual of the community to observe and examine similar phaenomena, to ascertain the weight that is due to every authority, and to state and discuss every question or doubt with perfect freedom. I have wished to give it that degree of authenticity and credit, which it cannot fail to obtain with candid and liberal men, from so fair a trial.

With respect to the nature of the information I have received in reply to my letter of the 22d of June 1793, which was forwarded to all the civil and military stations in Bengal, it is proper, before I conclude, to make the following remarks.

1st. That the officers of the army, to whom I am obliged for a large proportion of it, have perhaps better opportunities, and more powerful motives for attending to the changes which

happen in fevers, than any other class of men in India. Being constantly employed during the first years of their service in the most unhealthy corners of the country, remote from medical assistance, their success, reputation, health, and lives, and the lives of all around them, depend, often, on the medical skill which they may have acquired from their own attention.

2d. That, induced by the coolness of the air, and the clearness of the sky, to spend their evenings in the verandas of their quarters, or out on open terraces, and their doors and windows being constantly open during the night, for at least three quarters of the year, the different revolutions and phases of the moon become no less familiar to them, than the paroxysms and remissions of fevers to which they are subject; and when these have a mutual connexion it cannot easily escape their observation.

3d. That the attention paid to the full and change of the moon but especially that paid to the latter by sepoys of the Mahomedan faith, contributes to mark these periods, and to connect with them circumstances which occur in the course of the diseases, the dates of which would otherwise pass without a record.

4th. That the mustering and paying of the troops on a fixed day every month, and the regular relief of guards at stated intervals, together with other different tours of duty, all published and daily circulated in the book of orders, tend strongly to impress and fix dates in the memories of military men; and enable them to recollect the connexion between times and incidents with superior precision and certainty.

Whilst we are estimating the opportunities and means by which military gentlemen may have acquired and recorded the information which is so distinctly detailed in the following letters, it is natural to ask what could be their inducement to impose upon themselves, without compulsion, so much unprofitable trouble? *What but a natural attachment to truth ; a generous feeling for him whom they saw exerting himself to save it from oblivion for public use ; and a sentiment that it was not fair or honourable to withhold from him that assistance which was due to his labours, and which they had it in their power to give.*

F A C T S.

THE different Facts that appear to establish the truth of the proposition, which is the immediate object of this Volume, are contained in Replies to the following Letter. To these I have annexed the observations of Dr Lind and Dr Jackson, &c. with some extracts from observations I have made myself*; for which I claim, although they are the result of particular attention to this subject for many years, no greater share of credit or weight than what is due to the testimony of any individual in the following list.

“ DEAR SIR,

“ From several observations made within these few years, in different parts of the world, some attempts have been made to revive a doctrine of very great antiquity in medicine : viz. “ That in fevers, and also in other diseases, the human body is affected in a considerable degree by an influence connected with the revolutions of the sun and moon.”

“ Amongst a variety of facts adduced in support of this doctrine, the following being analogous to the phaenomena of the tides, appear to be the most striking and conclusive :

“ 1. That the paroxysms of fevers shew themselves in a greater degree of violence about the full and change of the moon, (that is to say, about three days and a half before and after, including at each period a space of about seven days) than during the intervals between these periods.

* These extracts being already printed in the first Treatises of this collection, are now omitted.

“ 2. That the paroxysms of fevers occurring during the periods described, are constantly more violent about mid-day and night, (that is to say, in the space included between half after eight in the morning, and half after three in the afternoon; and between half after eight in the evening, and half after three in the morning; including at each a space of about seven hours) than during the intervals between these spaces.

“ 3. That some remarkable abatement in the violence of the paroxysms never fails to take place upon the expiration of the periods of the full and change.

“ 4. That the paroxysms of fevers, whilst they abate in violence upon the expiration of the periods of the full and change, shift also their accession or attack to a later hour.

“ Various other circumstances and arguments have been stated, and one gentleman says, that his observations of solar influence have not been confined to fevers, but that epilepsy, insanity, paralytic affections, head-achs, inflammations of the eyes, asthmas, plethoric coughs, gout, rheumatism, tooth-achs, pains and swellings of the liver and spleen, diarrhoeas, cholics, costiveness, complaints in the urinary passages, boils, ulcers, sores, and eruptions of different kinds, often assumed an intermittent form, and returned regularly with the full and change of the moon; and disappeared or diminished during the intervals. He also conceived that the hemorrhoids, the fits of teething in children, and the delivery of women with child, are directed to happen more frequently at these periods than during the intervals, by the same law.

“ To determine whether there actually exists in nature a *general law* of so great power, or whether it be merely the *phantom* of a few individuals, must certainly appear to every man of sense and candour to be a matter of consequence, not only to medicine, but to every branch of natural knowledge.

“ From this persuasion I have been induced to undertake the collection of all the information I can obtain on this subject, and flattering myself that every lover of science will be ready to assist me with his testimony as far as it goes on either side of the question, I have taken the liberty of requesting the communication of any facts, that may appear to be connected with my present object.

“ That my History of Facts may be as fair and impartial as possible, I must also request your permission to publish whatever you communicate, in your own words, and with your name annexed †.

“ I shall hope to have the pleasure of hearing from you soon, and shall consider every observation with which you may favour me as an obligation.

“ I am, Dear Sir,

“ With esteem,

“ Your very obedient Servant,

“ FRANCIS BALFOUR.”

*Calcutta, 22d }
June, 1793. }*

† I know it for certain, and it will be readily believed, that the apprehension of having their letters exposed to the examination and criticism of the public, has deprived me of many correspondents, and of much valuable information; and it will naturally occur, that nothing but a full persuasion of the truth of what they had to communicate, could have encouraged those gentlemen who have favoured me with letters to overcome this objection.

REPLIES TO THE PRECEDING LETTER, &c.

No.	From	Resident in India	Observing at	Distant from the nearest tide
		<i>years</i>		<i>Miles.</i>
1	Lieutenant L. Hook	10	Ramnagur	365
2	Lieutenant A. Black	13	Silhet	150
3	Captain R. Ogle	24	Cooch Behar	270
4	Major James Pringle	24	Benares	365
5	Major S. Farmer	25	Midnapore	58
6	Lieutenant Robert Cumming	14	ditto	
7	Lieutenant S. Sinclair	14	ditto	
8	Lieutenant J. Hamilton	14	ditto	
9	Captain S. Knowles	24		
10	Mr William Chambers		Calcutta	
11	Mr W. Spottiswoode, A. S.	13		
12	Lieut. J. W. Hutchinson	14		
13	Major Robert Bruce	24	Cooch Behar	
14	Mr James Ross, A. S.	11	Dinajpore	160
15	Mr Adam Burt, A. S.	13		
16	Mr J. Vannorel, A. S.	3		
17	Mr J. G. Henderson, S.	14	Beejigur	360
18	Lieut. Frederick Marsden	14	Bencoolen	0
19	Mr Hugh Mair, H. S.	23		
20	Captain Bradley	24	Chunar	370
21	Mr Chas. Desbrough, A. S.	4		
22	Captain George Wood	23	Ranigur	240

REPLIES TO THE PRECEDING LETTER, &c.

NO.	From.	Resident in India	Observing at	Distant from the nearest tide.
		years		Miles.
23	Colonel George Deare	25	Calcutta	
24	Capt. Richard Grueber	23	Rohileund	660
25	Mr James Wilson, S.	13	Murshadabad	53
26	Mr John Gilchrist, A. S.	11		
27	Capt. John Rattray	24	Tellasore	
28	Mr Charles Todd, A. S.	11	Bauleah	70
29	Mr Charles Campbell, A. S.	4	Fort Marlbro'	
30	Mr P. Cochrane, S.	14		
31	Mr W. Baillie, A. S.	13		
32	Lieut. James Price	12	Chunar	370
33	Lieut. John Towers	12	Cawnpore	530
34	Lieut. Robert Dee	11	Chitterpore	300
35	Lieut. Thomas Brougham	10	Juanpore	400
36	Mr W. Davidson, A. S.	10	Silhet	150
37	Mr John Corse	11	Tipperah	50
38	Dr James Campbell, A. G.	9	Dinajpore	160
39	Mr John Miller	30	Calcutta	
40	Dr Alex. Campbell, S.	15	Calcutta	
41	Major Dickson	25	Cooch Behar	270
42	Mr W. F. Gardiner	17		
43	Mr W. Boyd, S.	10	Buxar	348
44	Mr W. Allison, A. S.	3		

REPLIES TO THE PRECEDING LETTER, &c.

N ^o .	From	Resident in India	Observing at	Distant from the nearest tide.
		years		Miles.
45	Major Dunn	25	Berhampore	47
46	Capt. N. Macleod	25	Cooch Behar	270
47	T. Henckel, Esq.	24	Jessore	
48	Mr James M'Dougal, A. S.	3	Dinapore	315
49	Mr John Farquhar			
50	Mr John Hannali		Calcutta	6
51	Dr Robert Bruce, S.	16	Lucknow	536
52	Mr W. Coote, A. S.	12		
53	Mr George Davidson	12	Dacca	
54	Dr N. Fontana, A. S.	11	Calcutta, &c.	6
55	Mr Tho. Bainbridge		Chittagong	6
56	Mr James Laird, S.	17	Dacca	
57	Mr Rob. Collins, S.	13	Rungpore	
58	Mr Ewart, A. S.	12		
59	Captain Dennis	25	Ramgur	240
60	Major A. Kydd		Pittebeāt	660
61	D. P. Wade, A. S.	12	Assam	
62	Mr Touchet	14		
63	Mr W. Dick, A. S.	13		
64	Dr Hare, A. S.	6		
65	Dr Boyd, H. S.	22		
66	Letters received after the conclusion of this list for press			

EXTRACTS FROM BOOKS, &c.

- 67 From Dr James Lind's Thesis.
- 68 From Dr Robert Jackson's Publications.
- 69 From Drs Blanc, Mosely, and Hunter's Treatises.
- 70 A Letter from Dr Davidson, Kinross.
- 71 Observations respecting the tendency of paroxysms to postpone the time of their attack.

The prosecution of these inquiries being interrupted by my return to England in 1798, and, after resuming my situation in Bengal in 1800, by want of leisure for carrying them on, they were then, for a while, relinquished. Observations, however, being still made and communicated from different quarters, I have taken the opportunity of inserting a few of those that appeared most material, as additional numbers to the original collection.

72. Contains letters that relate to the effects of sol-lunar influence in the island of *Ceylon*, *Sumatra*, and *Penang*; by Mr Christy, superintendant of Hospitals in his Majesty's service, and by Messrs Lumsdaine and Hutton, surgeons in the service of the Honourable Company.

73. Contains some very interesting observations made at the General Hospital of Calcutta respecting the effects of the horizontal parallaxes, by Mr Howison, and Drs. Fullarton and Campbell; and by Dr William Hunter, marine surgeon.

74. Contains an extract from the Edinburgh Magazine for June 1794, relating to the effects of the transition from the autumnal-equinoctial period, to the winter interval, in reducing, in a very extraordinary manner, the number of the sick at Philadelphia in America. I insert it here because it will serve to represent and confirm what I have observed during many years at the same season in Bengal.

75. A letter from Dr Helenus Scott, respecting Bombay and the western side of India.

Evidences of Sol-Lunar Influence in AFRICA.

1. An Extraet of Dr M'Gregor's Observations in Egypt. These observations which are communicated to the public, in a work entitled Medical Sketches of an Expedition to Egypt, and which is not at present in my possession, contribute essentially, not only to confirm the existence of that power, but to shew also, that the knowledge of its laws are indispensibly necessary to a judicious and successful practice in that country. But they are important and interesting in a view far more enlarged and extensive; by carrying on, by evidence that is unquestionable, the empire of sol-lunar power to the very climates where Hippocrates and Cleghorn wrote their inimitable histories of fevers,—even to GREECE and to SPAIN, it can be no longer doubted that these histories, bearing the perfect stamp and impression of sol-lunar influence, are the true and immutable forms of nature; and that the forms of fevers in the other quarters of Europe corresponding with these, are moulded and shaped by the same cause.

Under these circumstances it was impossible for me not to feel and deeply lament the unfortunate *omission* of Dr M'Gregor's *evidence* respecting the existence of sol-lunar influence in Egypt, in the account given of his work in the Edinburgh Medical Annals. By this unfortunate *omission*, however, being opportunely taught that the dominions of my celestial mistress can be preserved and extended only by measures that are vigorous and rapid, her *crescent* is now advanced from Abukir to Copenhagen, and will soon fly in the Castle of Edinburgh.

Evidences of Sol-Lunar Influence in EUROPE.

1. This number contains the history of a case at Liverpool, in which a most distressing pain returned every day in the morning, for the space of 26 years, (between 1780 and 1807,) and during that time was constantly increased at the periods of the new and full moon.

N. B.—The distances marked in the last column, are very nearly the number of miles in a direct line, between the places where the observations were made, and the utmost reach of the tides at the springs; and I have taken pains to ascertain them upon the first authority*; because Dr James Lind has lately advanced that “Lunar influence will be no where observed, but where the tides rising high at the springs overflow the land, and leave swamps;” to which cause he refers them.

The Monthly Reviewers, in their account of my first Treatise published at Calcutta, omitting the word *almost* in the first page, of course represent me affirming that the attacks and relapses of fevers happen *invariably* at the springs. It is owing, I should imagine, to this omission, that they and Dr Jackson have represented my observations to differ so materially from his.

By a reference to the Treatise itself, it will be found, that I have made use of the words “*almost invariably*,” and that afterwards, in page 4th of the same Treatise, I have explained my observations upon this point at full length in the following language. “Although I am now endeavouring to establish “the superior influence of the full and change of the moon “in producing bilious fevers, I must at the same time allow, “that the intervals are by no means exempted either from “first attacks or relapses,” &c.

I now think that the *intension or increase* of the moon’s influence continues *at least seven days* at the lunar periods.

* General Kydd, then Surveyor-General, and now at the head of the Engineer Corps in Bengal, had the goodness to adjust them at his office.

NO. 1.

HAVING experienced for several years, in this country, a series of bad health, occasioned chiefly by an affection of the liver and frequent attacks of fever; and having in that period watched with great attention the approaches of the springs, the result of my observations enables me to speak positively to the truth of the facts stated in the first and third articles of your letter of the 22d ultimo, by answering that, for several months in succession, I have felt either a return or an increase of fever, not only about the time of the new, but likewise of the full moon; and that the violence of these attacks was invariably diminished, or the fever entirely gone off, in the course of three or four days after the height of the springs.— With evidence so strong, I must be the veriest sceptic upon earth, were I to entertain a doubt of the influence of the moon on the human body, in fevers; at least of her being concerned in producing those periodical effects, which I have so often experienced. Nor is it optional, where proof is so clearly brought forward and supported by the evidence of the senses, to believe or not believe.

My observations have not been sufficiently minute and attentive to enable me to speak fully upon the facts stated in the second article of your letter. I can however venture to say, that, so often as I have taken notice, I have found that the paroxysms of the fever during the springs, were generally more violent about mid-day, even so late as four o'clock, than at any other time.

The facts stated in the fourth article of your letter, I can speak to from experience, having particularly attended to the circumstance in the latter end of the year 83, and beginning of 84, during a very severe illness, which terminated in a fever and ague. The difference of time between the return of the fever on one day, and that on the day following, was generally, I think, from half an hour to three quarters later.

I shall be extremely happy if any of these circumstances can be of the smallest use to you ; at all events you have my full permission to publish them, &c.

L. HOOK.

Fort William, 18th July, 1793.

P. S. A circumstance having occurred which prevented me from sending you this letter yesterday, I avail myself of the opportunity of adding one observation more on the subject ; which perhaps would with greater propriety have made part of the first paragraph of my letter, it being connected with, and appearing to be a further confirmation of the fact stated in the first article of your letter. It is that, during an obstinate fever which I contracted in the upper part of the country, and which having about me for nearly two years, I, without attending to the approaches of the full and change, have several times discovered the return of these periods by my own feelings and return of my fever.

Accompanying are my sentiments on the influence of the moon in fevers as far as I have observed.—Several years ago, and whilst I was suffering from that influence every full and change, I committed my sentiments on the subject, and sent them to a friend of mine of the faculty in Scotland. They were in substance the same as these now sent you, &c.

L. HOOK.

19th July, 1793.

NO. 2.

UPON being presented by Captain Mackenzie with your letter on the subject of Lunar Influence in Fevers, I found a desire to send you the enclosed narrative of some circumstances which passed under my own observation. I am perfectly conscious how very inadequate these observations must be to throw even a ray of light on the important subject which you

are prosecuting with so much credit to yourself and benefit to mankind. I am led, however insignificant they may appear, to add my mite, in the hope that you will believe it proceeds from an anxious desire to render all the information in my power to a person so meritoriously employed. But I have to request in the most particular manner, that you will not suffer my trifling remarks to be published. If you conceive them even worthy a perusal, my end is fully answered.

I am, with much esteem, &c.

A. BLACK.

Gyah, 15th July, 1793.

N. B.—The following are the remarks which were communicated by Mr Black, and which afterwards he permitted me to publish, with other observations contained in a second letter.

In the month of May, 1789, I accompanied a detachment, consisting of three officers and one hundred and fifty sepoy, who were ordered from Dacca to reinforce a detachment commanded by Lieut. Cheap in the Sylhet district; where they had been for some months before. The country in which Lieut. Cheap had hitherto been employed being very low, and always inundated during the rainy season, rendered it necessary to move to some higher spot. He accordingly made choice of a hill covered with thick jungle, and large forest trees, which after much trouble and labour were cleared away, as well as the nature and circumstances of the situation would admit of. Great numbers of the trees were very large, and as the rains set in, workmen could not be procured to cut them up, so as to admit of their being rolled away, consequently a great number remained on the spot where they were felled. Upon joining in the month of May, we found Lieut. Cheap's detachment very sickly. In the course of a few days, two of the officers were seized with fevers, and obliged to go to Sylhet, in order to procure medical assistance. In less

than a month, most of the sepoy's were attacked with a fever, which almost invariably came on about the full or change of the moon, and generally went off during the intervals. The first symptoms of this fever were, a tensive weight in the back of the head, lassitude, thirst, and the person generally complained of being disturbed at night by frightful dreams. In the month of June we were joined by a surgeon, Dr William Moore; I generally attended him every morning where the sick were collected; and, to the best of my recollection, the medicines which he generally administered, were first either emetic tartar or calomel; and afterwards bark in substance. These sometimes procured an intermission of the fever, but the patient invariably relapsed toward the full or change of the moon. I think generally rather before than after that period. Their relapses were almost in every instance accompanied with symptoms of a more alarming nature, such as inflammation of the liver and spleen, dysentery, swelled legs, and in many instances the face swelled and became pale and glossy. Some also were attacked with a most violent retching and purging, which frequently proved fatal.—It appears to me that those who used the bark most freely, were in general more subject to those violent symptoms, than those who used it more moderately. I know but one instance of any person recovering from this fever, without having experienced frequent relapses. This was a servant of my own, to whom, upon the first appearance of the fever, I had given some emetic tartar; but the man being naturally of a very weakly constitution, became in the course of a few days so extremely reduced, as made it appear to me dangerous to give him any active medicine.—I therefore resolved on giving him some emetic tartar in very small quantities.—I accordingly put four grains into a quart bottle of water, and gave him a small wine glass full, every hour from day break until evening.—It acted only as a purge; the man got better daily, is now in my service, and never had a return of the disorder. I mention this circumstance, because it appeared to me singular. In the

month of september I was ordered down to the presidency in charge of seven European artillery men, who had served with the detachment, and who were totally unfit for duty from repeated relapses. During the course of our passage to Dacca, I was seized with a shivering fit, succeeded with a smart fever, and violent perspiration; after which I had no return of the disorder for some months. When our boats put to on the evening of this day, I found that most of my servants, and all the artillery men had experienced a relapse of the fever. This circumstance appearing singular, I took a memorandum of the day of the month, and upon my arrival at Dacca, on referring to an Almanac, I found that the day on which it occurred was that which preceded the full moon. In the course of our passage from Dacca to the presidency, five of the men were attacked on the same day with a return of the fever; and I particularly remarked that this circumstance happened about the change of the moon. To the best of my recollection the fever went off in the course of four or five days. All these men were affected with inflammations of the liver and spleen, and many of them had swelled legs. I really do not particularly recollect at what time the paroxysms of this fever generally made their appearance. During my residence at Dacca, I was twice attacked with an ague and fever. In the first instance the paroxysms invariably came on every second day, between the hours of 11 and 12 o'clock at noon, and in the second instance, which continued 9 days successively, always between the hours of 1 and 2 o'clock in the morning.

A. BLACK.

SECOND LETTER FROM LIEUT. BLACK.

If you conceive that any part of the narrative can in the smallest degree tend to elucidate the subject of Lunar Influence, you have my hearty permission to make use of it in any manner you think proper; but as it was intended for your private inspection only, I must request that you will expunge whatever may appear to you extraneous, and also that you will

correct any inaccuracies which may occur.—I really do not recollect whether the two attacks of the fever which I had at Dacca, happened at the full or change of the moon. From cotemporary circumstances, I think the first happened about the 10th of January 1790, and the second about the 15th of October, of the same year. Indeed the truth of lunar influence in fevers was so strongly impressed on my mind, that I looked upon the attacks happening at either the full or change of the moon, more especially in that part of the world, as a matter of certainty. I was unpardonably negligent in not informing you that, during my residence at Berhampore, in 1791 and 1792, I was three times attacked with an intermitting fever, which invariably came on about the full moon. The surgeon who attended me during my last illness, Doctor Spottiswood, advised me to take two or three doses of bark for some days preceding and following the full and new moon. I accordingly did so for some months, and have not since had any return of the complaint. Since my arrival at this place, I had a fever which I attributed to having wantonly exposed myself to the sun in the month of April, when engaged in superintending some public works which Captain Mackenzie had entrusted to my care. It did not appear however, that the moon had any influence on this fever; and I had great reason to think that it was infectious, as Mrs Black was, soon after my recovery, attacked with a fever of a similar nature, and accompanied with the same symptoms. I am the more inclined to think, that lunar influence had not any effect on this fever, as several revolutions of the moon have occurred since that period, and no person has experienced a relapse. It appears to me, though conscious how ill qualified I am to give an opinion on the subject, that the influence of the moon in fevers is more particularly applicable to those which are contracted in the eastern parts of Bengal, or in countries much covered with wood, &c.

A. BLACK.

Gyah, 30th July, 1793.

NO. 3.

I was some days ago favoured with yours of the 26th ultimo, but I am sorry to say that my debilitated state from long indisposition, renders me **totally** unable, as also my mind too uncollected, to respond fully to your letter, having been confined to my bed, where I still am, these two months past; besides which, there are but four or five gentlemen of the battalion at Dacca with me, to whom I shewed your letter, and interrogated concerning your queries. But their answers were only in general terms, and rather vague and incoherent; that they believed such was the case, but had not paid particular attention, whether the full and change of the moon had any influence on the fever they had had or not, but understood such to be the case; and in general with most fevers in this country. In short, I could gather nothing from any body worth putting to paper. I have myself never attended any of the gentlemen at present with me in fevers, and indeed my own health has been so bad for some time past, that my mind is totally incapacitated to think or study much, yet I am perfectly assured myself of the reality of the moon's influence on fever in general; many remarks of which I made formerly, and submitted to paper, especially the two years I was at Cooch Behar; and as many afterwards upon the same fever, which were attendant on most of us, even that length of time after we left the country. But these, as well as many other remarks and observations, were burnt with every thing belonging to me at Cawnpore, in 1785, when my bungaloes were demolished by fire; since which, I have seldom troubled myself much about medical matters, except when necessity, from no professional man being present, obliged me. Most of the gentlemen whom I formerly attended are dispersed, gone home, or deceased, in Cooch Behar, as I believe you must yourself recollect. No man escaped the fever, except poor Sir Patrick Balfour; nor did it leave most of us for a long period after quitting that country. It was there the moon's influence on

the paroxysms of fevers first attracted my notice, and led me to make many observations on fevers afterwards, when I found that it had the same effect upon them in general. These fevers, after the first attack, generally returned two or three days before the full and change: though with several of the natives, also Lemond, Sandford, James Dickson, and myself, sometimes only at the change; and the violence of the paroxysms increased gradually for four, five, and sometimes six and seven days; when they gradually diminished in the violence of the fits, which were sometimes also attended, especially about the third day, with delirium, preceded by heaviness and head-ach. For the four or five first days, they came on twice in the twenty-four hours, more violent in the paroxysms or fits, especially in the evening; but this was not the case with all, for with some, especially the natives, the fever often intermitted for near a whole day, I mean twenty-four hours; and also with many, the fever was continual for two or three days together, and then left them almost entirely, after having reduced them to a very weak state in that small space of time. However, these variations were but with few; in general, the paroxysms were, as before mentioned, increased in violence on the fourth, fifth, and seventh day, generally most violent in the evening; towards the crisis, only once a day, and then late in the evening, or if in the morning hours, eleven o'clock, or near noon. These observations are all that I can recollect, and bring my mind to unburthen at present, and am afraid, my dear Balfour, indeed I am confident, are but indeterminate ideas, in comparison of what you wished from me. But at present I am too ill to recollect or lay down every thing more fully or explanatory, unless I had my papers to remind me and copy from. However, I never heard the reality of the moon's influence in fevers in this country yet doubted, as it occurs almost daily at a certain season of the year, when fevers frequently prevail. But probably Doctor Hamilton of my battalion will write you more fully and clearly on this subject, as he said he would in answer to your letter. In the mean time excuse my conclud-

ing this scrawl, as well as the many errors in it, for which I hope my sickness will apologize, &c.

ROBERT OGLE.

On the Barampooter, 16th July.

NO. 4.

I AM favoured with yours, containing the papers addressed to the gentlemen of the faculty. I received them when I was in company with one who coincided with you in the doctrine of lunar influence, but seemed averse to go into print. You may depend upon my circulating the papers among the faculty. As far as my memory assists, I will commit any facts relative to my long attack to paper, and transmit it to you.

JAMES PRINGLE.

Cawnpore, 12th July, 1793.

NO. 5.

IN answer to your letter of the 22d June last, I shall relate such matters as I can remember, respecting the accession, duration, and abatement, of the fevers, by which I have been very much afflicted at Midnapore for a long period, but particularly in the year 1791.

I did never notice at what time of the moon the first attacks have generally commenced; but the fever never failed to return at the next succeeding full or change, if there had not been, which was seldom if ever the case, a sufficient interval of time for medicine to remove it entirely. It further occurs to my recollection, that the fever was much more violent, if returning a day or two previous to the full or change, than if it had come on two or three days after either of these periods; and it was so established an observation of the more prevalent influence of the moon at those particular periods, that myself, as well as all others, I believe, afflicted with the fever, even af-

ter having escaped its return, continued for some time afterwards to use preventives at these periods of the moon, and these periods only.

I do not recollect seeing an instance of the fever's attacking or returning in the night time; the first attacks were generally from about eight to ten o'clock in the morning, the paroxysms continuing generally five hours the first day; but as the fever gave way to medicine, the accession became postponed two, or sometimes three hours later than at the preceding paroxysms; and then abated in violence and duration in proportion to the periods of their later accession.

I never had occasion, nor gave opportunity, to notice whether the paroxysms of the fever abated in violence at the expiration of the third or fourth day subsequent to full or change, independent of the effect of medicine; but I never remember that, having wholly escaped the return of the fever, at either the full or change of the moon, I ever experienced an attack in the interval between the one or the other of these periods.

S. FARMER.

Hazari Baug, 21st June, 1793.

NO. 6.

I HAD many fevers at Midnapore, and, as well as I can now remember, they were in general, if not always, influenced by the full and change of the moon; that is, I had commonly the first attacks of them in one of the three days preceding or following these periods, but oftener those preceding them. The relapse I have observed also to happen about the return of these periods, unless prevented by medicine given during the interval. I have observed the same in many cases, both Europeans and natives. I cannot speak as to the precise number.

I cannot recollect, at this distance of time, the exact hour

of the day or night in which I was commonly seized with the fever; but, to the best of my recollection, it was oftener in the morning and evening than at any other time.

Some of the fevers I had were very violent, and some of them much less so, which I imputed to the greater or less degree of relaxation I laboured under when the fever attacked me, and to my then regular or irregular state of body. The fever itself seemed to be peculiar to the place; for a change to a purer air generally removed it. I ought to have observed before, that it was a practice with many, who had suffered most from repeated attacks of the fever, to take two or three doses of bark only, beginning a day or two before the commencement of the periods of full and change, and continuing it till their termination, by way of prevention.

RICHARD CUMMING.

No. 7.

I HAVE had many attacks of the fever when this battalion was at Dinagepore, and also at Midnapore, and am of opinion they were almost always influenced by the full or change of the moon, and have observed that the fits came on more frequently in one of the three days preceding, than in any of the three days after those periods. The fever most commonly came on in the day time, between the hours of seven in the morning and two in the afternoon. I observed that the fevers which came on in the forenoon were more violent, and of longer duration, than those that came on in the afternoon. Latterly, when at Midnapore, the fever came on about the commencement of the second quarter of the moon; I had five or six relapses of that kind, but none of them near so severe as any of those before mentioned, and never preceded by the cold fit. The last attack was in September 1792, since which I have not had any fever.

J. SINCLAIR,

NO. 8.

I HAD frequent attacks of the fever when with this battalion at Midnapore, and am of opinion they were in general greatly influenced by the full and change of the moon; and have observed, as well as I can now recollect, that the fits most commonly came on between eight in the morning and two in the afternoon. I am also of opinion, from experience, that a change of air frequently prevented a return of the fever at the time it was expected.

JOHN HAMILTON.

NO. 9.

I DO myself the honour of acknowledging the receipt of your favour of the 2d instant, covering a printed paper containing sundry queries on the subject of the lately revived doctrine of Lunar Influence on the human constitution, &c. &c. with a requisition that, by circulating the said paper among the officers of the corps I command, I would endeavour to procure as many testimonies as possible in further illustration of that celebrated theory.

With respect to myself, every doubt has long since vanished; for though I shall not hesitate to declare that I have, for the greatest part of my life, been a pretty rigid observer of the precept enjoined Adam by the seraph Raphael, viz. to confine his attention to terrestrial objects, as better adapted to his intellectual strength, under the full assurance that the planetary system stood in no sort of need of his interference, so that no possible advantage could result from his vain attempts to investigate the sublime and incomprehensible arcana; yet have I been long enough a valetudinarian to assert, with every necessary degree of confidence, that the moon has, at the periods you mention, a more than ordinary effect on the patient, under almost every complaint I have experienced, and I believe the contents of Pandora's box have been nearly ex-

hausted upon me. To descend to more minute particulars is at present not in my power, as I have ever considered the discussion of such points more particularly belonging to professional men, whose business, and even duty, it is to make and to promulgate such observations as may in any degree contribute to the improvement of science, to the consequent enlargement of the human understanding.

SAMUEL KNOWLES.

16th July.

NO. 10.

If I had been of your profession, what you have written would certainly have led me to make particular observations respecting the influence of the moon on the human frame; whatever concerns that is important, and if only one life should be lost, or one man's health materially injured, in the course of a physician's practice, by an inattention to a point of so obvious a nature, and so easy to be ascertained, I think he ought not to forgive himself. That there is an influence, lunatics, and other well known phaenomena, plainly testify. I should suppose, therefore, that, if a difference of opinion exists upon the subject, it must relate to the degree of this influence rather than to the general proposition. As to myself, I really can point out nothing that would not better suit a desultory conversation than a letter, for I have not data enough to establish a single item with certainty.—I believe I told you that I knew a person who was worthy of credit, who had often occasion to observe a wakefulness both in children and adults, but especially the former, at the new and full moon; and I have been in company where others have agreed in the observation, but I cannot venture to be circumstantial.

WILLIAM CHAMBERS.

Calcutta, 27th July, 1793.

No. 11.

I HAVE been favoured with your letter, and it would afford me much satisfaction could I furnish you with any observations on the points you are endeavouring to have ascertained respecting sol-lunar influence in various complaints, but am sorry I have none to communicate to you of sufficient consequence for publication.

I have not for a long time entertained any doubts of the existence of the general law, and of its influence being often manifested in fevers, particularly by the tendency to relapses at the full and change. The precaution which this suggests I have found of great consequence in practice, viz. of recurring to remedies at these periods, even after they were left off in the intervals, and continuing them in this manner until strength was completely re-established. It is very common to meet with patients, convalescent from intermittents, who probably never read a medical book, and are therefore not influenced in favour of any particular theory, sufficiently convinced from their own experience, *i. e.* sufferings, of the necessity of attending to the above precaution, and of carefully avoiding all irregularities at these periods. Its being a matter of common observation amongst such, is no small proof of this influence prevailing in a remarkable degree in Bengal. I think also every practitioner in this country must have remarked, that medicines are much more liable to fail in the effects expected from them at those times than in the intervals, *i. e.* that there is often a difficulty in interrupting the succession of paroxysms during the springs, and that frequently it cannot be done completely till they are over, when a continuation of the same remedies soon completes the cure.

WILLIAM SPOTTISWOODE.

Burragong, July 18th, 1793.

No. 12.

HAVING had for many years successively what in India is

termed a jungle fever, I frequently experienced the effect to the sun and moon in that disorder.

The influence of the first I generally felt about the time of the autumnal equinox, at which periods the fever commonly attacked me. At the full and change of the latter I observed, that in whatever stage of the disorder they happened, the paroxysms of the fever were then severer than at any other times, but particularly so at the full of the moon, a day or two before, or a day or two after.

J. W. HUTCHINSON.

Cawnpore, 19th July, 1793.

NO. 13.

I HAVE received your queries respecting the influence of the moon in fevers, and in reply to them I have to inform you, that I commanded a party of artillery in Cooch Behar in 1773, when there were employed at the same time, in the reduction of that country, two battalions of sepoys. In the course of the rainy season most of the European officers and a great number of the men were seized with fevers, subject to frequent relapses a long time after the first attack; and I recollect perfectly, that it was the general opinion that these relapses were brought on by the full and change of the moon, and that it was common for the officers to take remedies to prevent their return at these times.

I was fortunate enough to escape the fever myself, but I particularly remember that Sir P. Balfour and myself were the only officers of that detachment who did escape it; and that you used to impute my good health to the constant and great exercise which I always took. The only person alive of the detachment of artillery I had on that service, is a Mr Wood, who is now a conductor of ordnance at Monghier; and of the two battalions that were on that service, I believe the

present Major Dickson and Captain Ogle are the only survivors in India. Your old friend Alston died two or three years ago; and, during his life, was always subject to returns of what in Cooch Behar, we used to call the jungle fever.

I recollect that bark was one of the medicines you gave your patients in great quantities, and that several of my artillery-men, who were obstinate and would not take bark, died very suddenly, to the best of my recollection within the twenty-four hours after their being first seized with the fever.

ROBERT BRUCE.

Fort William, 1st August, 1793.

NO. 14.

I WAS favoured with your printed letter yesterday, together with what you were pleased to write in addition, which I lose no time in doing myself the pleasure to answer.

It appears an unaccountable, though common lot of the mob, whether rich or poor, high or low, to patronise or promote with ardour or avidity objects of the most absurd and nugatory nature, provided they be showy and of easy comprehension; while subjects pertaining to science or art, which hold forth a less gaudy exterior, and though attaching to themselves the most profound erudition or most acute ingenuity, yet, in so far as in these their best commendations they require some thought to comprehend, or sagacity to evolve, this rabble will run down at full cry, and do their best to worry. Truths of the greatest magnitude have accordingly, in all ages, been treated with much ungenerous scepticism, and many of the utmost importance to the human species have, I fear, in this vulgar ignorance, been totally rejected; and thence, most probably, have arisen the eras of learning and darkness which have alternately enlightened, and peopled, or reduced to wastes, the spacious face of this our terrestrial

globe.—Socrates and Galen, Copernicus and Galilæo, Harvey and Brucc, have each been subjected in their turns to the severity of persecution, or scoff of ridicule, for having proposed facts, or asserted truths, which now, or will soon, form the Catholic Creeds of all who may pretend to learning or liberality. Nor should you, my good Sir, be ashamed, if mankind might make you the object of their temporary obloquy for inculcating the planetary influence on the human frame, a doctrine which had the pretty general support of those most admirable observers of nature, the Greek and Roman Physicians; many of whom will shine for ages to come, the luminaries of their profession. The ancients, in the circumstance, I think, of a mild and more steady climate, had a more convenient opportunity of attending to these uniform operations of nature, which they in general have, as in all her more simple facts, been most happy in detailing.—It has not surprised me to see the physicians in the more northern parts of Europe, with the almost singular exception of our own most excellent Cullen, treating this doctrine as rather a chimerical refinement than useful branch of the healing art. But it astonishes me to find, that gentlemen of education, who have in these and similar parts of the British dominions better opportunities perhaps than the ancients of attending to this subject, should so generally doubt the solar and lunar influence in fevers and other acute diseases; to which their frail and mortal brethren, whether black or white, are all subject. Though probably fortunate themselves in not having felt any personal illnesses that could have attracted their serious and rational attentions, I wonder their patients, who in this country are many of them men of superior penetration to those the faculty practise on in other parts, have not made them aware of it. For my own part, and a broken constitution is my woeful proof, I unluckily cannot plead *Seipsum ignorum Morborum Pandoræ*; though, had it been otherwise, I must nevertheless have been convinced by the remarks of many intelligent patients, who have repeatedly, in my hearing, foretold the

accession of their remittent and intermittent fevers on the change and completion of the moon. Indeed I can see no solid reason why the humours and air circulating within the human body, should be exempted from a solar and lunar influence, any more than the same humours and air, in their more august state of oceans, clouds, and atmosphere, whose periodical vicissitudes, as connected with and dependant on these two bodies, the moon and sun, none I believe now a days are hardy enough to doubt. Though it be hazardous to refer to events which took place some years ago, more especially when you do not retain any memorandums or records to refresh and strengthen your memory, yet I am satisfied that in the course of my practice I have remarked the effects of both the sun and moon in fevers, &c. on the coast of Africa, among the West Indian Islands, and in South America, as well as in almost every corner of Bengal and its more immediate dependencies. If I recollect aright, for it is many years since I read it, your first pamphlet upon the Lunar Influence was the result of observations made on a military expedition towards the hills, to the northward of this place. The critics of late have been so very fastidious in admitting any dependence being put upon this fact, that I was led a little time ago to conceive it might, like the source of rivers and storms we have in these parts, have some connection with the hills and mountains that tower into the skies behind us. But a moment's recollection of what I had observed in champaign countrys, some time before I had seen, and could of course be supposed to be prejudiced by your book, convinced me I had no occasion to argue partially on what was clearly, if any, a general law of nature. Be this as it may, soon after my arrival at this station I remarked the stated recurrence of this influence in fevers, fluxes, and a species of bloody phlegmon, which is a very common and painful complaint during the rains; my comments upon this subject I recorded regularly for two or three years together, with a diary of the weather and other connected remarks; but unfortunately, during an official trip over to Rungpore, a rat

was pleased to deposit her young among my papers, and the drawer having been afterwards shut, she devoured and destroyed the whole. This accident, and other avocations, which I have recurred to in my despondence of ever arriving at any rank in my line, have entirely withdrawn my mind from professional studies for these three years past, nor is it likely I shall ever recur to them with much ardour. But in compliance with your attentive requisition, I have deemed it a polite return to state the above general and irregular detail; which I trust, but cannot promise, to be able to digest, and add to it any observations I may make of the nature you wish, by the middle of the ensuing cold weather; which, as far as I have observed, I know not if owing to the frequency of sickness, embraced the period during which this planetary influence would seem in the greatest degree to predominate.—You may depend you have my best wishes in this your arduous undertaking to confute prejudice and assert the truth. But whether I can follow up this letter with any trifling observations I can hereafter make, depends so much upon circumstances, that I would decline making any promises. I am conscious of being able to do my duty in relieving, as far as our art will admit, any sick I may have under my care; but it is one thing to practise, and another to commit this to such writing as may bear the nice eye of criticism; and I confess, what some might reprehend, that I would rather leave a thing undone than not excel in it.—&c. &c.

JAMES ROSS.

Dinagapore, 6th August, 1793.

NO. 15.

I was favoured with your letter yesterday, my answer shall be candid.

Since the first publication of your observations on the Influence of the Moon in Fevers, I have not been inattentive to the subject. But my attention has been perhaps less par-

tiacular than the matter merited, and my distinction in the service, for some years past, has not presented me a practice sufficiently extensive to enable me to form myself a conclusion altogether satisfactory on this head. Since I did duty at the General Hospital in 1785, I have met with but few cases of pure intermittents. In the district of Ramgur, where I was stationed for some years, fevers prevailed in very many places towards the end of the rainy season, and continue more or less during all the cold weather. That country is hilly, wild, and much covered with wood, but not abounding with stagnant water, nor marshes. The soil where the worst fevers prevail, teems with iron, sulphur, and pyritous strata in a state of spontaneous decomposition, and the fevers seemingly engendered from the effluvia of minerals, evidently depend on local circumstances; and their phaenomena never appeared to me to obey the lunar influence.

In other fevers, particularly intermittents, the frequency of exacerbation, and the violent recurrence of paroxysms at, or near the periods of the full and change of the moon, have, in this country, struck my observation very forcibly. At the same time, however, I must own, that in protracted fevers among the natives, where the course of the disease is little or not at all interrupted by medicine, its violence does not always decline proportionally at the intervals equidistant from the above-mentioned period; and some cases I have known, tho' comparatively few, where the accession and exacerbations of fevers took place on days remote from the periods stated.—Such exceptions might seem to oppose the idea of a general law in regard to the sol-lunar influence in fevers analogous to that on tides; but then we ought to reflect that there must be a material difference between the *modus operandi* of any principle on dead and on living matters. In the latter case, the external causes of disease, as well as the incidental changes in the body itself, must often operate so as to modify, favour, or counteract, any general influence.

Upon the whole, my observation of facts goes to support the doctrine which yours so powerfully tend to establish ; but, except in febrile cases, the lunar influence has never appeared to me clearly perceptible.

In this quarter, the exacerbations of fevers and the renewal of paroxysms, happen, I think, most frequently in the afternoon, and abate after midnight.

Should future observations enable me to amend my report, I will gladly communicate the result to you, being at all times, with very true regard, &c.

ADAM BURT.

Ghya, 6th August, 1793.

No. 16.

As death almost invariably occurs during the exacerbation and paroxysms of remittents and intermittents, and since these are found to sympathise so much with the changes of the moon, it would induce me to suppose that death may happen most frequently at those changes.

At the periods you have mentioned, I have no doubt that the system is more obnoxious to the action of the febrile remote causes, as well as from conversing with gentlemen in this country who have long been in the habit of studying the effects of the changes of the moon on their constitutions. But how far the moon may influence local and other disorders unconnected with the endemics, I shall not presume to form an opinion. Its influence, however, would appear to me to be equable on the system by inducing a state of increased susceptibility, which favours the action of those morbid causes that are so constantly applied to the body of every one in this country ; particularly at the places, and at the season, the atmosphere is most loaded with the exhalations of marshes, &c. I would consider it merely as a predisposing cause to disease,

not capable of itself, without the assistance of the remote causes, whether epidemical, or whether contagious, to excite disease. The convalescent and the irritable are those who feel the effects of these changes most, and it would seem, particularly in those labouring under fever, to impair the vital principle.

I hope your present liberal address will operate as a proper stimulus to medical men to collect every fact that can throw light on this important subject. I now fear I may have intruded too much on your time and patience, therefore shall conclude by assuring you, that no pains or attention shall be wanting on my part, &c.

JOHN VAUMOREL.

Gya, 27th July, 1793.

NO. 17.

I AM favoured with your letter requesting me to communicate any observations I may have made on the influence of the moon in fevers. Some years ago, when I was surgeon to a native corps of seven hundred men stationed at Beedzy Gur, where remitting and intermitting fevers prevail much both amongst the inhabitants of the district and strangers, during the months of August, September, and October, I constantly found a great increase in the number of sick of the corps, at the full and change of the moon, and I observed them as regularly to decrease during the intervals between these periods. I generally had upwards of two hundred men in the hospital during these months, and more than two-thirds of the cases were intermitting fevers. The register which I kept of the number of sick, their disorders, and the period of attack, for five years of the time I was at that station, fully evinced the lunar influence in the fevers that prevail in that country. I have unfortunately either lost or mislaid these papers. Fluxes did not prevail much at Beedzy Gur. I had a fever myself

there which attacked in the month of September about noon, and continued with an intermission of six hours out of the twenty-four for fourteen days; and from that period I seldom escaped an attack of the fever at the full and change of the twelve months; although I generally commenced taking the bark a few days before, and continued it during these periods; and being generally costive, took some purgative before I took the bark. My health was afterwards re-established by a change of air. Lieut. Hind, of the artillery, told me yesterday, that he was at the siege of Beedzy Gur, and had a fever every day for three weeks, which intermitted about twelve hours, and that he had an attack of it at the full and change upwards of a year. He says he got quit of it by using the cold bath. I am sorry I cannot give you any particular cases. What I have mentioned is from memory.

J. G. HENDERSON.

August 8th, 1793.

NO. 18.

HAPPY in meeting the wishes of a professional man, anxious to establish a point of such consequence as the moon's apparent influence on fevers—I with great cheerfulness communicate a few observations made at Bencoolen in 1780 and 1781.

Soon after my arrival there from Bengal, I was attacked with a violent fever. Its duration I forget, but well remember, that in extremity my physician told a friend, that, as it would be new moon the following day, I should either die or recover at that period. A favourable change took place; I continued to mend, though very slowly, and a quantity of bark which was thought necessary, and thrown in for I believe three weeks or a month, induced, as I imagined, an affection of the spleen, which to this hour continues; and was for nine years attended by an intermittent fever, during which time the lunar influence at the full and change was surprisingly regular, though not

uniform in its effects. I also perfectly recollect that it was the opinion of many persons in that settlement, that fevers were more or less violent according to the approach of the above periods.

FREDERICK MARSDEN.

Barrackpore, 8th August 1793.

No. 19.

I WAS favoured with yours about the middle of last month, with some enclosures that were delivered as directed. At that time I was so much indisposed, and have been ever since, that I have not been able to answer it sooner, and indeed the few observations that I had an opportunity of making on the moon's influence are scarcely worth communicating. They have however produced on my own mind a thorough conviction and belief, that several diseases are very much affected, if not occasioned by it. For example, the relapses of intermittent fevers happen so regularly at the times of the full and change of the moon, that I know of no cause to which they can be imputed, unless it is to the moon's increased influence at such seasons. The relapse commonly takes place about forty-eight hours before the change and the full, and the paroxysms are generally at their greatest state of violence at noon and midnight.

In some cases of epilepsy which I have had under my care, I observed that the paroxysm generally happened on the very day of the full or of the change.

In cases of insanity, I suppose no person will doubt the moon's influence. I have known some persons in this country of irritable habits, who lose two or three nights sleep at the full and change, without any obvious disease.

HUGH MAIR.

Dinapore, August 5th, 1793.

No. 20.

I RECEIVED your letter, and should have replied sooner, but some of the officers being absent I waited to communicate it to them, several of whom are in their own minds convinced of the existence of the lunar influence on the body; but not having made a regular series of observations, are prevented making any written communication.

As to myself, I am fully assured of it, having frequently experienced it, more so in general at the change than the full, and the greatest effect in the twenty-four hours after that had taken place.

When I joined the 29th battalion of sepoys in 1788, I had just then returned from the command at Dinagepore, where numbers of them had been detached into the low and jungly parts of the country; so that there were at times more than two hundred sick. They had in general recovered when I arrived at Chunar, but having every now and then observed an extraordinary increase of the sick list, without any alteration of station or particular exposure of the people, I made inquiry concerning it, and found that the additional sick were generally men who had been severely attacked with what they called the Dinagepore fever. Although relapses did occur at every period of the month, yet at times there were six or seven of a day, which naturally excited my attention; and in consequence I observed, that it was generally about the full and change of the moon. These, by the attention of Mr Fraser, the surgeon of the battalion, soon got well, and I am sorry that he is not here to give you his sentiments on the subject, as I am convinced he has had as good an opportunity of judging of it as most men in the country. Any thing that may occur to me in future, on this subject, I shall with pleasure communicate to you.

J. BRADLEY.

Barrackpore, August 11th, 1793.

No. 21.

I WAS favoured with your printed and written letters three or four days ago ; and though it is not in my power to answer them in the manner you might expect, viz. by a communication of facts relative to the doctrine of sol-lunar influence on the human body in fevers, &c. &c. yet it is nevertheless incumbent upon me to acknowledge the receipt of your letters, and to thank you for the attention you have shewn me, even though it be in common with others, by addressing me on this occasion.

I am sorry that I am not furnished with any evidence in support of the doctrine in question, since you desire to collect as many proofs as possible of its truth and validity. I can only say, that it has my suffrage ; it is a part of my medical faith, no doubt. But I might have collected numerous observations on the phaenomena of its operation ; and in fact, I recollect that I have made them, in the course of my attendance on sick persons, though tacit ones, and never committed to paper ; partly through a shameful negligence perhaps, and partly,* principally I should hope, because I conceived the doctrines to be firmly and universally established throughout the medical world. It is a theory so agreeable to all animate nature, and so consonant with all human reason, its practical utility and importance so palpable and notorious, that it is to me like a self-evident proposition. It may escape observation, but being presented to view, must needs be recognized.

CHARLES DESBOROUGH.

Sewarah, 4th August, 1793.

No. 22.

I HAVE received your letter of the 22d June last, and with much pleasure will afford you every information, as far as my observations, and the recollection of them, touching the subject of it, extend.

In the year 1788, when stationed with the 2d battalion of sepoy's in the vicinity of Ramghur, a general sickness prevailed; the complaint an intermitting fever of a very malignant nature, which commenced with the rains, and continued during that season to extend its influence, with few exceptions, to every person in camp.

As I had occasion to distribute medicines to many sepoy's of the battalion, my own servants, and others of the camp followers, I did observe, and do well recollect, they were generally seized with the fever at the commencement of the springs, at the full or change, viz. about three or four days before either periods. If four days, they usually, the first day, complained of a head-ach, had a slight shivering and a short hot fit; and the next had a more violent one, which, if not checked by medicine, attacked them daily during the springs, and appeared to me to increase in violence, till the day of full or change; after which it perceptibly decreased in length and severity, and left them the fourth or fifth day, including a space of seven or eight days, seldom more.

I do not recollect any instance of this fever's continuing during the intervals betwixt full and change, which are denominated the neaps.—An observation I made, but my recollection is not so clear as to speak decidedly to it, was, that those men who were attacked at the first of the springs on the full moon, generally escaped till the same time next full, and so of the change. I hope what I have hitherto stated will answer your first and third positions; I did not attend the paroxysms with sufficient accuracy to speak decidedly on your second.

As I was in the practice of seeing the men mix and take their medicine, and had occasion for this purpose frequently to send for them before the attack came on, I did observe the truth of your fourth position, so attentively as to speak deci-

dedly to it, viz. that paroxysms of the fever mentioned, did shift their accession or attack to a later hour.

My own case will, perhaps, be as little amusing or interesting to the public, as my observations on those of others ; but if it can in any measure contribute to elucidate, or give weight to any position of yours, I give it cheerfully.

During my abode at Chitterpore, I was once attacked with the intermittent, but by a timely emetic, on the first sensation of cold, gave a complete check to that fit ; and a few drops of antimonial wine, as a sudorific, considerably shortened the hot one. I had, as far as I recollect, three paroxysms. At the approach of each, I observed the above practice, which prevented a fourth ; and by the use of a little bark, I never afterwards, during my stay there, had any fever.

On my marching from Chitterpore, the second day's march I had a regular fit of the intermittent, and by the next day's exertion of walking a march of about twenty-one miles, with a view to baffle another, the fever turned into a nervous one, and I was for four or five days in a very low state, without any perceptible pulse. All the time I took plentiful doses of bark, laudanum, and wine, which so far braced me, as to enable me to mount the sixth day, and take my post with the battalion. On my arrival at Dinapore, I had symptoms of a liver complaint, which increased on our reaching Chunar, where I went through a regular course of mercury, internally, but without any effect in removing or radically curing me ; for on the setting in of the rainy season, I was again very sensibly afflicted with it, and began an external application of mercury on my right side, which I continued for more than three months, but without any permanent efficacy. On this I was advised to try a sea voyage, and came to Calcutta for that purpose ; but on the cold season setting in I found myself so much braced, and the painful sensation in my right side so trifling, that I was

induced to give up my sea trip, and to march by stages to join my corps at Cawnpore, where on my arrival I found myself so much improved in my health, that I am convinced, had I continued in that climate a year or two, I should have got entirely the better of my liver complaint.

The battalion was soon after ordered to fall back to Chunar, and from that marched to Juanpore, where I was again seized with symptoms of the liver, and most violently on our return to Chunar, where I again went through a mercurial course externally and internally. The event was a most virulent bilious fever, the latter end of September, from which I scarcely recovered my strength during the whole cold season. You know the rest, and my being again seized with a bilious fever in September last.

I have only to remark, that ever since I have been afflicted with the liver, I am regularly at the commencement of the springs, at every full and change of the moon, seized with a very painful and uncomfortable sensation of fulness in my stomach and bowels. My urine, during the period of the springs, is turbid and of a deep brandy tinge; my habit costive, and my spirits dull and heavy. In the intervals, or neaps, the disagreeable fulness is very little, or at all, troublesome; urine clear, but not too limpid; habit regular, and my spirits cheerful.

Many, both officers and men, of the battalion were, for some seasons after leaving Chitterpore, annually seized with the intermittent, and, as far as I observed and can recollect, were generally in the paroxysms during the springs. It may perhaps be useful to remark here, that very few either of the officers, or men, were attacked with this intermittent, the season of their march to Cawnpore, but had it the season afterwards on falling back to the Chunar station.

GEORGE WOOD.

Dinapore, 9th August, 1793.

No. 23.

IN reply to your questions respecting what I may have observed, or heard, of the influence of the moon in fevers, I can give you but little information. My friend, Colonel Duff, used to have severe fevers almost every rainy season when in Bengal. Being one of his family for several years, I had an opportunity of observing his complaint in the different stages of it, as at times I used to attend him day and night. I recollect that the late Doctor Daniel Campbell, who attended him in one of them, always expected a relapse at the full and change of the moon, and I gave him medicine to prevent its violence at these times; and I am perfectly convinced Duff was always worse at these periods than at any time of the moon; and I recollect he himself always expected to be so.

I have observed that many of my military friends who had what we call the jungle, or hill fevers, used at the change and full of the moon to have it more or less, for years after they were first attacked with the complaint.

C. G. DEARE.

August, 16th, 1793.

P. S. I have mentioned Colonel Duff in particular, but I know it to be the general opinion which prevails. I have heard many gentlemen who have been ill, talk of the influence of the moon in fevers with much confidence, from their own observation.

No. 24.

I HAVE the pleasure to receive your favour, requesting the communication of any fact within my knowledge, that may appear connected with your inquiries respecting the influence of the moon in intermittent fevers.

I have considered it an established axiom for many years

past, that persons attacked with intermittent fevers, or, what we who have suffered from them emphatically stile jungle, have invariably felt more or less the severity of the disease, on the full or change of the moon. And the following circumstances originally gave rise to my belief on this subject. About fifteen years since, I was one of a detachment serving in Rohileund. The number of officers was, I think, twenty-six. We were on that duty twenty-two months, and, as well as I can at this distance of time recollect, a third of us did not escape fevers, the paroxysms of which were undoubtedly influenced by the moon. I have myself experienced it, and one of my servants is still afflicted by the same description of fever, contracted at the aforementioned time and place. The sixth, seventh, and eighth years of the disorder's progress, I expected the man would have sunk under its pressure. Since that time, however, there has been a regular abatement, and greater distance of time in the attacks of the complaint: yet the paroxysms have been regulated constantly by the moon.

RICHARD GRUEBER.

Allypore, August 10th, 1793.

NO. 25.

I FEEL much obliged to you for the attention you pay me in supposing that any observations of mine can be at all useful to you. I have too much knowledge of my own abilities, and too poor an opinion of them, to offer any observation of mine to the world, but if they can have privately any power to strengthen you in your opinion with respect to any influence the moon may have upon the human body, I will give them with much satisfaction.

1st, "That the paroxysms of all fevers shew themselves "more about the full and change of the moon than at any other period." I am perfectly conscious of the justness of this observation from long practice; but particularly so since your first publication on this subject, which led me to see the

great utility we might receive from such inquiries: for it being once ascertained that the moon really has such influence, it must afford us very great powers not only to lessen the symptoms, but perhaps in some cases prevent them altogether. I can with much truth say, that such observations have given me very great success, particularly in fever, in head-ach, pains and swelling of the spleen and liver, and in fluxes, which last generally arise from some defect in the liver; and that such symptoms have always increased as the moon approached to full or change, and lessened between these periods, and in some cases totally disappeared till such change again took place.

2d, "With respect to the time of paroxysms within the "twenty-four hours." I have generally observed the attacks to commence between the hours of eight in the morning and eleven in the forenoon. But whether the attack is more violent at such period than between the hours of three in the afternoon and eight at night, I have not made the distinction.

3d, "That some remarkable abatement in the violence of "the paroxysms never fails to take place upon the expiration "of full and change." This observation is particularly just, and has in general given me so great a reliance upon the fact, that I have repeatedly ventured to assure my patient of certain relief from his complaints on such a day; meaning that the expiration of the full or change would take place on that day.

4th, "That the paroxysms of fever whilst they abate in violence upon the expiration of full or change, shift also their "attack to a later hour." This observation holds equally good with any of the former.

I have not met many cases of stone in this country. Whilst I was in Calcutta our friend Mr ———, had a very severe attack of it, attended by his old fever, on the commencement

of the change. On the expiration, his symptoms lessened; yet never quitted him entirely during the period between the change and full. But on the commencement of the latter, they increased to a very alarming degree; and on the day of the full he continued in excruciating pain, attended with cold sweats, from two o'clock in the afternoon till half past six in the evening, when he had a slight fainting fit, and on coming out of it he said he was easy. About nine o'clock the same night he passed a piece of stone, and in the course of the night three more smaller than the first; after which he continued to recover. I know of no case I have ever met with, which ascertains more the influence of the moon upon the human body, than a fever Mr —— has been plagued with for many years, and I believe he is as much convinced of its power as any medical gentleman in India can be.

J. WILSON.

Moorshadabad, 20th August, 1793.

NO. 26.

I TRUST, after this candid confession, you will forgive my inability to speak on the subject of your letter dated 22d June, from my own personal experience. I however can furnish you with the extract of a letter from a medical friend, who perused yours and wrote me thereon.

“I believe it is generally understood, and indeed a well known fact, that the moon influences, in a great degree, all maniacs; and from what I have observed in fevers, particularly of the intermittent kind, I think there is not a doubt but that it affects and has a very powerful influence over them. Paroxysms of fevers certainly do shew themselves more violent about the full and change of the moon than at any other time. In the fevers which I have been subject to myself, the paroxysms were always most violent at midday and mid-night, and I am convinced they are so in most people. The parox-

ysms were always less violent upon the expiration of the full and change, and shifted their accession to a later hour." The gentleman who writes the above has had considerable experience in his profession, and is on the whole a man of knowledge and observation, but from a false delicacy will not let me send his name on this occasion. Were I at all qualified to speak with precision on the point in question, I might add, that the generality of the medical men with whom I have conversed seemed to support your doctrine from their practice; and most of the intelligent patients in the various kinds of intermittent fevers, who have stated their cases afterwards to me, were inclined to support the same from their own immediate experience. How far this very circumstance, however, of expecting the accession or return of the paroxysm at a particular and known period, may hereafter be reckoned among the number of predisposing causes of fever in weak and irritable habits, I leave you to determine; as it may yet put practitioners on their guard against making patients too knowing, and particularly in the attack and progress of such fevers as the moon is supposed to influence most, so that the apprehensions of a paroxysm may not baffle the very remedies employed at the time to keep it off: and among these, in my opinion, there is none more powerful, in many otherwise obstinate intermittents, than a change of air, pursuits, &c. for a few weeks.

JOHN GILCHRIST.

Ghazipore, 23d August, 1793.

NO. 27.

I WAS favoured a few days ago with your letter enclosing one for Mr Meick, which I delivered to him, and he told me he would reply to it soon.

There is a fact which may not have come within your observation, but which, as you can at any time ascertain, I beg leave to mention.

There are many people in the Guttal and Jellasore districts, who are troubled with a swelling in their legs, a disorder generally known by the name of the Cochin leg, and which I am told is most common amongst the weavers, probably owing to their sedentary employment: but of the cause, you will on inquiry be better able to judge yourself. What I have to observe is, that those afflicted with this malady, complain of its recurring with great violence at the full and change of the moon, at these periods being seized with most acute pain in the affected limb, attended with a smart fever, so as to be totally unfit for any kind of business. But as the springs take off, the pain and fever gradually leave them, when they are again enabled to pursue their usual avocations. Mr Touchet, at Guttal, will be able to give you further information on this head, as I have heard him mention this curious fact. It has been since confirmed by several of the country people with whom I have conversed on the subject; as also by one of my own officers, who was lately on command in the Jellasore district.

JOHN RATTRAY.

Midnapore, 28th August, 1793.

No. 28.

I HAVE to apologize for not sooner acknowledging your letter on the subject of Sol-Lunar Influence, with which I was duly favoured the beginning of the present month. But a desire to furnish the memorandums you required occasioned the delay; and the motive will, I trust, obtain me your indulgence. Permit me to assure you, that I should have the greatest pleasure in complying fully with your wishes in this instance, and am extremely concerned that it is not in my power to do so at present: my common note-book having been unluckily lost or mislaid, during my late visit to the presidency, and all my endeavours to recover it having hitherto proved ineffectual. Any circumstances that may hereafter occur, tending in any

manner to illustrate the doctrine in question, I shall have much satisfaction in immediately communicating to you ; and in the mean time, I beg leave to add my assent to the general propositions, and to reply to your queries respecting intermittents. Agues are at times extremely prevalent in this neighbourhood, especially towards the close of the rainy and commencement of the cold season ; and though it is not possible to be always very accurate in my observations, owing to the want of any regular establishment, and the consequent detached mode of practice among the natives, on the veracity of whose reports, when left to themselves, very little reliance can ever be placed, yet the two following facts may, I think, be safely affirmed.

1st. That not only in true intermittent, but fevers of every denomination, the influence of the moon is in some degree perceptible ; and that the first attack very generally happens about the time of the full and change.

2d. That the paroxysms most usually come on between the hours of nine and two o'clock in the morning or evening ; more frequently I think in the forenoon ; and more severe, and of longer duration, at the full and change, than in the intermediate period. Exceptions undoubtedly occur as well in regard to the accession of fever as of the paroxysms, but they are not sufficient, in my opinion, to invalidate the general doctrine ; and among several patients labouring under quotidians and tertians, whose residence in the vicinity of the factory enabled me to attend to them regularly, I scarcely recollect an instance of late, when they have not been first attacked nearly about the full or change of the moon, or when the accession of the paroxysms varied more than from nine to one o'clock.

CHARLES TODD,

Bauleah, 28th August, 1793.

No. 29.

I AM noting the endemic disorders of the country, the various influence to which they are subjected, and among the rest, as the most powerful, the sol-lunar. From studying this with attention, I have acquired some small reputation by predicting, with considerable accuracy, the accession and decline of fevers; and repeated experience has convinced almost every body here of its power.

CHARLES CAMPBELL.

Fort Marlborough, 31st July, 1793.

No. 30.

I HAVE been favoured with yours of the 22d June, and, tho' fully persuaded of the influence of the moon in fevers and other distempers, yet I have not made my observations with sufficient accuracy to be laid before the public.

P. COCKRANE.

Cawnpore, 18th August, 1793.

No. 31.

I HAVE been favoured with yours, requesting to know if I had ever made any observation in the course of my practice, respecting the influence of the moon in fevers and other complaints in this country. In answer to which, I may safely say that, from the repeated observations I have made, I am perfectly convinced that the influence of the moon, at the change and full, has evidently shewn itself in a very great number of cases of fevers, both continued and intermittent; that is, in a continued fever, the paroxysms have been more violent; and, in an intermittent, it was more apt to return, and on so doing to remain in a stronger degree, during the influence above mentioned, than it would at the intermediate periods of the moon's revolution. To prove, almost beyond doubt, the influence, with respect to intermitting fevers, it will be found a good

practice to give the patient, in proper time, such medicine as may resist or counteract the lunar influence at the expected periods. Of the advantage of this practice, I have often seen unequivocal proof. I have also remarked, that people with feverish habits, and liver cases, attended with a good deal of fever, are often worse at the full and change of the moon. But in liver cases unattended with fever, I cannot say that I ever perceived the effect of lunar influence; nor indeed, in any other cases than those I have mentioned. Should any case, however, occur in the course of future practice, that may throw any new light on the subject in question, so as to be sufficiently interesting for your information, you may depend on my communicating the same to you. Before I conclude, I beg leave to mention a case that I thought very remarkable; and, if it is not fancy, evinces the lunar influence in a curious manner. A gentleman, who had suffered much from fever in this country, and, in consequence, had acquired a feverish tendency, assured me that he was obliged to guard himself against a moon-light night with as much caution as against the mid-day sun, though the moon might not be at the full; and that from the time he had used the precaution of avoiding moon-light, and, particularly, excluding it from his bed-chamber, he had got the better of his feverish habit. When I saw him last, he was in perfect health.

W. BAILLIE.

Berhampore, 11th September, 1793.

NO. 32.

IN the month of May 1788, while the 22d battalion to which I am attached was stationed at Chitterpore in the Ramgur district, I was first attacked with a fever, which is generally called the hill fever, and which assumed an intermittent form; and until the end of September of the same year, had a relapse every full and change. The fits generally came on about eight or nine o'clock in the morning, and left me about

two or three o'clock in the afternoon; except in the last, the paroxysms of which did not shew itself until three in the afternoon, and left me about midnight. The second day intermitted; the third day the fever returned, with violence, about nine in the morning; and I was not sensible of any abatement until the eighth day in the evening. The next day the fever left me so much reduced, that I was unable to move.

The fever was always preceded by pain in the hip, joints, arms, hands, and legs, a great heat and numbness in the fingers; and sometimes a very acute pain over the eyes and back of the head. I was at Gyah in November, at Dinapore in December, and arrived at Chunar in January, 1789. At these places the fever returned every full moon, and until the end of February; as also at the full moons in May and July at Juanpore, either on the day the moon was at full, or on the day after; but I cannot call to mind at what hours. In November I marched from Juanpore for Cawnpore, and also from Cawnpore in March, and arrived at Juanpore in April 1790. In May I entered on a course of medicine for obstruction in the bowels. In June the fever returned (for the last time.) In consequence I obtained a certificate, and on the arrival of the battalion at Chunar in August, proceeded by water to the presidency, which I left in December, and arrived at Chittagong in January 1791; stayed there till July; arrived at Juanpore in October; and continued in the districts under the Benares presidency until the beginning of January last, when I marched to this station. During my residence in the hills, I was induced to believe, from the return of fever at the stated periods of full and change, that the moon had influence in producing the attacks and relapses; and I am more confirmed in this belief, from the numberless instances which I have since observed in servants and others, male and female. During the time I was stationed in the hills, my servants were generally sick at the same time I was; and for a considerable time after, until the fever entirely left them, had the return at full

and change. I have had frequent opportunities of making the same remarks on the sepoy, when in the lines and the field of exercise.

Although I have not had any return of fever since June 1790, yet I have frequently at full and change a pain in the head and the eyes, numbness and heat in the fingers, and sometimes a pain in the liver.

In June 1790, I made some memorandums from memory, which I believe are correct, and from which the above account of the fever is taken.

JAMES PRICE.

Futty Ghur, August, 1793.

NO. 33.

ABOUT June, 1788, I was attacked by a fever of that kind usually called in this country the hill, or jungle fever. I was at that time stationed at Chitterpore, in the Ramgur district. After the first violence of the disorder had abated, it settled into an intermittent, accompanied by ague, and of that form termed tertian. It returned regularly at the full and change of the moon from June to January following. During that time I do not recollect once escaping; and what I conceive as a further corroboration of lunar influence in the disorder is, that I, as well as two or three other gentlemen, observed the second and last quarters, or neaps, to have similar effects, though the symptoms were but slight: the paroxysms being more severe, and returning more regularly every second day for about two or three days before, and three days after the full and change of the moon.

Except in the first attack, when the fever had assumed no settled form and was almost continued, I do not recollect experiencing a fit before eight o'clock in the morning, or after

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three in the afternoon. For the first two or three months the paroxysm commenced about half past one or two o'clock in the afternoon; and what impresses it more strongly upon my recollection is, that, as we used to assemble to dinner about that hour, those who were well needed no other testimony of the moon's phases than the number of those assembled! for it was observed out of a company of six or seven who were accustomed to dine together daily, on the approach of the full and change of the moon, seldom more than three were able to meet; and if by accident more did assemble, one or two were generally obliged to leave the table before dinner was half over.

After some unavailing delay in hopes of meeting with relief from medicine, experience demonstrated its fallacy, and the impossibility of recovery, or in fact of mitigating the disorder, except by a change of air; I in consequence came to Danapore. Every attack subsequent to my arrival there, began from about eight to ten in the morning; and this was uniformly the case every full and change to the first day of my arrival at Chunargur, the middle of January, 1789, when I had a most severe but final attack. Since then I have never had any regular return of the complaint. Indeed, in November last I had two attacks of fever and ague, two days intervening between each; but which I attribute to my having been in a very weak state, and exposed to the sun during the heat of the day for several hours.

Never having thoroughly recovered the effects of the fever, which renders me susceptible of bilious attacks and indigestion, and having constitutionally weak nerves, I have hitherto invariably found at the approach and during the cold season, on the full and change of the moon, a tendency to fever: that is, dryness of skin, unusual heat, and fulness of pulse, succeeded by chilliness. During these periods, my nerves have ever been very much disordered; and I have frequently found a

dull burning kind of pain in my bones, and mostly in those of my legs and thighs: a pain better conceived than described by those who have been so unfortunate as to experience this malady.

While I was in the hills, six or seven of my servants were afflicted with fever. I have no particular data to go by, having neglected to take memorandums at the time; but from having attended them during their illness, I can assert, positively, that the moon at the full and change seemed to possess influence in their cases, similar to that which I had experienced in mine. There were also eight gentlemen ill of fever at the same time I was, and who, in all the instances wherein I had opportunity of observing, were attacked only at the full and change of the moon.

The following are the names of the places where I have known the moon at the full and change to possess influence in cases of intermittent fever: Chitterpore, Chittra, Gyah, Patnah, Danapore, Chunargur, Juanpore, Allahabad, and Cawnpore.

JOHN TOWERS.

Futtygur, 25th July, 1793.

NO. 34.

ABOUT three years ago, Mr Wade, who is now with Captain Welsh's detachment, made me a similar request to yours, and desired I would inform him of any observation I might have made respecting the fever I had caught at Chitterpore, near Rangur, about two years prior to the above period. The circumstances being then fresh in my memory, I was enabled to state them with tolerable correctness, and to comply with Mr Wade's request. I am sorry I cannot do myself the pleasure of answering your's in the same manner, neither having kept a copy, nor being able from length of time to recollect

[more than the substance of what I stated to Mr Wade. However, if you think it will be of any use, I have not a doubt but you may obtain a copy by applying to him. I had many relapses, and remember perfectly well that they came regularly on, with scarcely an exception, about the period of the full and change of the moon. The observations I made upon those of my servants who were ill, were the same as in my own case; and the gentlemen of the 2d battalion, who were attacked, experienced similar effects.

ROBERT DEE.

Futtygur, 5th August, 1793.

NO. 35.

I AM much concerned it does not, at present, lay in my power to give Mr Balfour such a particular account of my late fever, as I either could wish, or perhaps such as will prove satisfactory. However, the point which Mr Balfour appears the most desirous to have answered, is so strongly impressed upon my memory, that I can without hesitation reply to it.

I invariably observed that the paroxysms were more violent two or three days both before and after the full and change of the moon; and more particularly so on the day of the full or change, than at any other period; and this was not confined to myself, for it also was the case with several of my servants who had the same kind of fever.

At the request of Mr Wade I wrote him a statement of my illness, and traced the several symptoms from its commencement, together with the mode of its being treated until I came under his own directions; and I should suppose Mr Wade can have no objection to Mr Balfour's perusing it.

I was ill of the fever both at Chitterpore and Juanpore.

T. BROUGHAM.

No. 36.

I AM favoured with your letter of the 22d June, a few days ago, and must regret my not having it in my power to give you any satisfactory account of the influence of the sun and moon in intermittent fevers, entirely owing to my not having taken notice of this particular circumstance. On perusing your pamphlet on this subject, some years ago, when fevers and agues were pretty frequent in this district, I was led to attend to the particular effects of the moon on this disorder; but from the best of my recollection I found no difficulty in stopping the paroxysms at any time more than at another. I am extremely sorry that I neglected taking notes at the time, which precludes me the pleasure of now sending you the diary; nor can I at this juncture say, with any degree of certainty, at what period of the moon they prevail, or at what hour the fit generally makes its appearance.

I have postponed answering your letter until this time, that I might have the pleasure of mentioning a few cases. There is now in the hospital five sepoys with a quotidian, two were admitted on the 29th ultimo, and three on the 1st instant; two have the paroxysms at day-light, two in the evening, and one at midnight; and, as far as my observations have been, I by no means conceive the paroxysms are more violent at this period than they were on their being first admitted.

A servant of my own, whose case I think deserves some little attention, I will take the liberty of relating to you: he had been for some days labouring under a bilious complaint, attended with a cough and violent head-ach. On the 2d, about 11 A. M. he was seized with a pretty severe attack of an ague; on the 3d, 4th, and 5th, the paroxysms were much the same as the 2d; on the 7th, new moon, the head-ach better, cough disappeared, and the fit did not make its appearance before one o'clock P. M. and much milder; on the 7th, about the

same hour as yesterday, his head-ach returned, but no paroxysms; the 8th and 9th free of complaints, only weakness. I have the honour to be, &c. &c.

W. DAVIDSON.

Sylhat, 9th August, 1793.

NO. 37.

THE hope of being able to reply to your letter of the 22d of June last, in a manner that might prove satisfactory to both of us, has in some measure been the cause of my delaying so long to acknowledge the receipt thereof

At the time I received your very polite favour, I was absent from this station, and did not return till the middle of last month, when I set about making some arrangement in the sick list, that might enable me to give at least clear and decided answers to some of your queries, respecting agues, the disease I meant to make more particularly the object of my attention. Very few, however, occurred till within these last ten days; and as the relief of the troops here took place on the 10th instant, by the arrival of a detachment from Chittagong, I am not clear how far the bad weather they experienced, during their march, may be considered as a principal cause of their illness. Of several taken ill since their arrival, certain it is that most, if not all, were seized with the fever during the spring tides: and it may also be considered as a corroborating proof of lunar influence, that several of the prisoners were seized with fevers about the same time. But even to people less disposed to be sceptical than the literati in Europe, one or two lunar periods would not be deemed sufficient proof to establish any doctrine. I shall therefore not trouble you with any particulars just now, but shall collect every fact *pro* and *con* that may come under my observation, till the beginning of November next, when the sickly season will be over, and shall then with pleasure lay the result before you. I must however ob-

serve, that I cannot take into account a number of patients who apply occasionally for assistance, as their attendance is very uncertain, and the accounts given by them vague and incorrect, especially if their illness has continued more than a few days.

I shall confine myself entirely to such cases as occur among the troops and the prisoners, whose regular attendance I can insure. We have here near 100 of the former, and about 150 of the latter; and by carefully noting down such cases as may happen among them, I hope to be able to give some satisfaction in the report I propose making. It may however be proper to add, that I am a believer in lunar influence, from many cases that have occurred to myself. After such a declaration, it may be necessary for you to make some allowances. But at the same time I do assure you that, should any bias appear in my report, it shall not be intentional: on the contrary, I will ever be more guarded than if I had never formed an opinion on the subject, or spoke to any of the natives, with whom lunar influence, in several diseases, is held to be matter of fact so notorious, that they have not an idea that any reasonable man can doubt it. I most sincerely hope before this cold season is over, that you may have something more substantial to occupy your attention, than collecting proofs of a doctrine, which, in fevers, I considered as established: for, of all pursuits in this country, I must say that neither learning nor abilities are encouraged; and that, exclusive of the love of fame, there are few worldly incitements, particularly for medical men, to aspire at eminence in their profession, &c.

JOHN CORSE.

Tipperah, 27th September, 1793.

NO. 38.

HAVING it in my power at any time to reply to your letter of the 22d of June, I have procrastinated from day to day the

information I always meant to give you on the subject of your present inquiry.

It was my intention to begin with a detail of several particulars observed many years ago, when I had a great number of fevers under my charge, and when I had no suspicion of sol-lunar power. But as the immediate object of your present volume is to establish the existence of such a law, I shall confine myself solely to the circumstances which made me reflect on the importance of your doctrine, at a time when there did not exist in my mind the smallest prepossession in its favour; and I will even acknowledge, that it forced itself upon me in spite of some degree of prejudice entertained against it.

In 1787, soon after my arrival in Calcutta, there occurred in my own family two very distinct cases of remitting fever. At what particular period these fevers commenced I did not observe; nor did I form any opinion from perceiving that the symptoms became violent during the spring-tides, and did not subside till the arrival of the neaps. But it struck me not a little to discover that, for many months after this first attack, one of these patients was constantly subject to a tendency to fever, marked by those appearances and sensations which are always observed to precede the regular paroxysm of remittents. These constantly went off with the return of the neaps.

If a *jue des mots* would not derogate too much from the importance of your subject, I could inform you that I have oc-cular demonstration of the truth of your doctrine, in the case of a patient who is at this time under my care. It is an inflammation of the eye, (apparently originating from relaxation,) which increases constantly at the full and change of the moon.

JAMES CAMPBELL,

Calcutta, 7th February, 1794.

No. 39.

FEELING an earnest desire to contribute, in however small a degree it may be, to promote useful knowledge, I take the liberty to send you a note of the following facts.

In the year 1758 I was in Bengal, where joy for the re-establishment of the British power, and an overflow of riches, made every person happy. It produced much festivity, and increased the disposition to hospitality, for which the inhabitants of Calcutta had always been famous. I joined the current; my want of experience, and the conviviality which prevailed, unavoidably led me into a compliance with the general practice; and I lived too freely, though by no means riotously. The consequence was, that I got a severe fit of the piles. I left Calcutta March 25th, for Madras, where I did not arrive until July the 5th. The disease increased on getting to sea, and for two months was so severe, that, for the greatest part of that time, I could not be in any other than a recumbent posture. I perfectly recollect that the pain was so very great, and particularly for about six or seven days at the new and full moon, that I was frequently delirious from 11 P. M. until 3 A. M. I had no relief from medicine, and the surgeon spoke of danger in a surgical operation. I therefore had no remedy but a rigid diet, and abstinence from spirituous and fermented liquors.—This at last got me ease; but I was frequently liable to a relapse; and when the fits were of any continuance, which they generally were, I observed that I suffered most pain on the new and full moon.

In the year 1771 and 72, I was attacked by a dysentery about the 20th of May in each year, which continued with different degrees of violence until November.—I recollect that I was worst on the springs, and particularly in the month of August and September, in 1772, I was reduced to a most deplorable state: but I well remember that the degree of suffering

was much more owing to some freedom or impropriety as to diet, than to any thing else.

I have seldom had any long and severe illness since 1772; but whenever I had any feverish complaints, which occasionally continued some time, I found myself worse on the springs. And I have found it necessary, as have several of my acquaintances, to be more regular and exact in taking the bark, at the new and full moon, than at other times. I remain with great esteem, &c.

JOHN MILLER.

Calcutta, 10th October, 1793.

No. 40.

I AM glad to hear that you are soon to favour the world with the information you have been collecting with so much industry, respecting the action of sol-lunar influence; and it appears to me to be a truth so well established, and of so much importance in medicine, that I give you my testimony with great confidence and satisfaction.

The circumstances which have confirmed my belief, are the relapses which continue to return for so long a period after the first attacks of remitting and intermitting fevers. These I know from my own observation, and the testimony of almost every person I have ever heard on this subject, are constantly connected with the full and change of the moon, and their recovery with the intervals between these periods.

About fourteen years ago, I lived with my friend Doctor Daniel Campbell, then surgeon general, resident in India for many years, and employed in a very extensive and successful practice: and I can assure you, that he was a firm believer in sol-lunar influence, and attended to it in his practice.

In short the operation of this law is confirmed by observa-

tions in every quarter of Bengal ; and I am informed by good authority, that it shews itself very conspicuously in a fever which prevails about Masulipatam.

ALEX. CAMPBELL.

Calcutta, 8th February, 1794.

No. 41.

I HAVE perused your letter addressed to me, with much attention, relative to the lunar influence in fevers, and other diseases ; and have to regret, that my observations have never been directed in so particular a manner, as to justify any opinion of mine upon a subject so interesting to society. All that I shall therefore attempt is, to state such circumstances as myself have experienced, with such observations as I have occasionally and accidentally heard from others, during a long residence in India.

In the year 1773, being on service on Cooch Behar, I was seized with a severe fever, generally called in this country, I believe, the hill fever; and to describe its particular malignancy, often termed the Cooch Behar fever ; as more than three fourths of the detachment, both officers and men, were infected with it, great numbers of whom never recovered.

The fever with which I was seized was at first most violent, and attended with delirium ; but at what particular period it became intermittent, I do not now remember. But this I perfectly recollect, that it became intermittent for many months, and that by taking large quantities of bark, it was checked and kept off ; except at the change and full of the moon, when I never failed to have one or two fits of the cold and hot fever ; nor could it ever be vanquished until I got to sea on my way to Europe, which was about twenty months after its commencement.

In the course of my residence in this country, I have it in

most perfect recollection, that I have frequently heard gentlemen say, "I have got a return of my fever to day; I shall have a return, as the moon is at the change, or will be at the full," &c. and it is my firm belief, that few gentlemen, who have been long in this country, have not occasionally heard the same, or similar observations, made by their friends and acquaintances, &c.

JAMES DICKSON.

Calcutta, 20th October, 1793.

NO. 42.

I WAS duly favoured with your letter, which I deferred replying to, in hopes of obtaining from some of the officers here, such account of their own cases as could have been submitted to the public. But as all of them, although fully sensible of the influence of the moon in intermittent fevers, have only their memory to rely on, I found their accounts so vague and undetermined as could have added nothing new to the proofs you have already with so much accuracy committed to the press.

In regard to myself, although the opinions you have supported have long guided my practice in fevers, I have not lately been in the habit of taking notes; nor had it occurred to me how far the influence of the moon might affect other disorders. This I will in future be more particular in observing, and I will not fail to communicate to you the result, &c.

J. GARDINER.

Futty Ghur, 16th October, 1793.

NO. 43.

MY whole time having of late been occupied by various avocations, I flatter myself you will do me the justice to attribute my delay in replying to your letter, not to negligence, but to its real cause.

In the course of my experience in this country, I have met with numberless instances of the truth of that doctrine, which you have so well illustrated in your ingenious essay; and am firmly persuaded, that an influence connected with the revolutions of the sun and moon, has a very sensible effect on febrile affections of the human body, both of the idiopathic and sympathetic kind, and particularly in exhausted and irritable habits. And if I have not observed the operation of the influence in all the instances, and to the same extent in which it is thought by some to exist, let this be ascribed to my inattention, or want of opportunity, rather than suspect the accuracy of others, on a subject which they have in a more particular manner made the object of their inquiry. Independent however of positive facts and observations, I should think an inquisitive mind, even in reasoning *a priori* on this subject, would naturally infer, that frail animals, living at the bottom of an atmosphere, must necessarily be affected by agents far less powerful, than that mighty force which heaves the ocean from its bed. Instead then of disputing about the existence of phaenomena sufficiently obvious to attentive observation, let us endeavour to investigate their cause, and the extent of its operation, that we may, if possible, learn how it may be counteracted. Can this influence be the effect of attraction, heat, putrefaction, or electric fluid? or are we to ascribe it to some hitherto unknown power? Without troubling you at present with fruitless conjecture, I beg you will believe me to be very respectfully, &c.

WILLIAM BOYD.

NO. 44.

I HAVE been some time favoured with your letter of the 22d of June, which from the nature of its subject, in a particular manner, claims an answer. I am sorry, however, that I have not hitherto been able to collect facts sufficient to authorize me to speak with any tolerable degree of precision on the subject. But, as far as my observations have gone, it has ap-

peared to me, that the moon has no immediate or direct influence in aggravating or producing disease. It must, I think, at the same time be admitted, that fevers more frequently begin their attack, or if already commenced, are more severe in their paroxysms about the full and change of the moon, than during the interval between these periods. But this has not appeared to me to arise from any direct effect of the moon on our bodies, but from a change which at those periods is almost instantly produced in the temperature of the air, viz. from a certain degree of heat and dryness to a state colder and moister. And this, by altering for a time the determination of the blood from the external parts to the internal, may, I apprehend, in some degree account for fever more readily taking place at those times in those predisposed to fever; and perhaps a variety of other disorders according to their several predispositions.

WILLIAM ALLISON.

Nattore, 26th October, 1793.

No. 45.

I AM sorry that the time will not admit of my replying to your letter so fully as I could wish. I shall briefly state what came under my immediate observations.

In 1773-4 the corps to which I belonged being ordered into the Midnapore jungles, we were instantly attacked by a bilious fever, peculiar to that part of the country; which continued to intermit for two, three, and sometimes four days.—On our arrival at Berhampore, it came regularly at every change of the moon; and continued so for eighteen months.—Major Hawkins, Captain Hearsey, but I need not particularise, for every officer in the corps was in the same situation, and in the same manner affected; and for my own part, I

constantly felt these changes until my return to Europe in 1787.

JAMES DÜNN.

9th November, 1793.

No. 46.

REGARDING the subject of your letter to me, which I have laid before the officers of the 13th battalion, I have no doubt myself, that the moon influences people subject to fevers. In 1774 I was very ill of a fever I got in Cooch Bèyhar; a climate notoriously hostile to all constitutions, especially during the rainy season,—I was forced by sickness to repair to Berhampore, where the disease for a long time baffled every effort of medicine.—After the severity of the fever had abated, I could not but remark, that for three years, during which I was in some degree subject to it, I was certain of an attack every change and every full moon. I have conversed with many officers who laboured under the same complaint, and they agreed, that they were likewise affected during the changes of the moon. I shall leave it to scientific men to account for this; but I cannot in my own mind have the least doubt of the fact, proved as it has been to me by my own experience, during so long a period as I have mentioned.

N. M'LEOD.

P. S. I do not believe that there can be any difference of opinion to contradict the influence of the moon in the fever contracted in Cooch Beyhar; many of the officers who have been on that service, and who have fully experienced its influence, are now alive.

No. 47.

I HAVE really found myself totally disqualified for the arrangement of those remarks and observations, upon the influence of the moon, which it was my wish to have forwarded

to you; and which by experience and long-suffering I could have stated beyond apprehension or doubt, in such a manner as might have rendered them worthy of publication. I am therefore reluctantly compelled to postpone the accomplishment till some future opportunity.

T. HENCKEL.

No. 48.

SOME time ago I had the honour of your letter, wherein you are pleased to request the communication of whatever facts and observations may have occurred to me relative to the influence which the revolutions of the sun and moon have on fevers and other diseases.

My residence in India has as yet not been of sufficient duration to enable me to produce facts which, when added to those recorded by others, might have helped to decide the present important question, whether there actually exists in nature a general law of so great powers, or whether it be merely a phantom of a few individuals.

Although I have never so particularly directed my attention to this subject, as to be able to give a detail of particular instances, where the influence of the moon was or was not distinctly observed at the periods you have so accurately laid down in your late work, yet, from the transient general observations which I have occasionally made, I am fully convinced, that in fevers the moon at the full and change exerts an influence by which the paroxysms of fevers are rendered more violent, longer in duration, and succeeded by more imperfect intermissions or remissions, than during the intervals between these periods.

This opinion is founded on my having observed, whilst proceeding by water with a detachment of Europeans from Calcutta to Dinapore, during the months of August and Septem-

ber 1791, that the number of those attacked with symptoms of fevers at the full and change of the moon, exceeded, by more than one half, those attacked during the interlunar intervals.

Owing, I suppose, to the more northern latitude and dryness of the soil, fevers of the remittent and putrid kind have occurred so seldom at Cawnpore (at least since I have been there) as not to afford sufficient opportunity of ascertaining, whether these disorders are at that place influenced by the moon or not. This far, however, I observed, that during the month of August last, the number of patients admitted into the hospital of Cawnpore with feverish symptoms, at the periods of the moon, were much greater than during the intervals. The symptoms in general, on admission, were such as chiefly indicated a collection of bile, &c. in the stomach and bowels; and in most cases, by the timely assistance of gentle emetics and cathartics, disappeared, and but very seldom assumed a regular remittent or intermittent form.

As there are no facts or circumstances in this letter, which are worth laying before the public, I must request of you to be so good as not to publish it, as my intention solely in addressing you at present, was because I thought it a duty incumbent on me, as well as a proper mark of respect due to your character, to answer your letter, even although I was conscious that I had nothing worth communicating respecting the doctrine of sol-lunar influence.

JAMES M'DOWGAL.

NO. 49.

IN compliance with your request to communicate such facts as may have come to my knowledge respecting the influence of the sun and moon on the human body, and particularly in my own case while labouring under an obstinate tertian fever, I have the pleasure to give you the following account of my

K k

disorder ; and I am sorry for not having been more attentive in comparing the returns of such other intermittents as I have had opportunities to observe, with the situation of the heavenly bodies.

I do not recollect at what time of the moon I suffered the first attack of the fever. The time of the day was about one o'clock in the afternoon, and the returns were extremely regular at the same hour every third day, for the space of nearly two months, notwithstanding the great quantities of bark I had taken. Every paroxysm terminated in a profuse perspiration, and was entirely over by five o'clock. During the space of five months afterwards I had returns only of the fever, but regularly at every new and full moon generally two fits, although sometimes only one. The first paroxysms of the return generally happened one day before the time of the syzygie, sometimes on the very day, but never later. The time of the day continued the same, as I never had a paroxysm later than two o'clock.

My physicians at last, seeing no prospect of affecting a cure by the ordinary means, advised me to make a sea voyage ; and I had at sea only one slight fit, and never after any return of the fever.

Another case which has lately come to my knowledge is that of Lieutenant Wagstaffe, of the engineer corps, who labours at present under a regular tertian, the first fit of which attacked him on the day before new moon ; and he has suffered relapses at the syzygies ever since, notwithstanding that he has been always prepared to take the bark for some days before. He passed Bankyazar on the 15th of last month, and had not had a fit since the former new moon : but he had one on the next day, which was that of the full moon.

You likewise desire me to give you some account of the regular diurnal variations of the barometer which take place in

this country, and which I said I conceived to be peculiar to tropical climates, from the otherwise unaccountable silence of every author whose work I had been able to consult on the subject. The first, intimation of this was from Mr Henry Trail, who informed me, that he had observed the mercury to rise every night till about 11 o'clock, when it became stationary. I immediately repeated his observations, and found that the fact was certain; but that there was likewise another diurnal variation which had escaped his notice. After numerous observations, at all hours during the day and night, I found that the mercury is subject to the following variations, with the utmost degree of regularity, throughout the whole year. From 6 in the morning till between 7 and 8 it is stationary; it then rises till 9, sometimes, though rarely till 10, when it remains stationary till noon; it then descends, and is lowest at 3, and continues stationary till 8; when it begins to rise, and continues till 11, and is then at the same height that it was at 9 in the morning.

On relating the above observations to the late Colonel Pearce, an indefatigable and rigidly accurate observer, and who had devoted much time and attention to barometrical pursuits, he was surprised that such regular variations of the mercury should have escaped his observation: but some time after, with great candour acknowledged the certainty of the fact; and framed an hypothesis to account for it, which you will probably be able to obtain on an application to Captain Grace.

To me the phaenomena appear inexplicable on any hypothesis that I can think of. The periods are evidently connected with the earth's diurnal motion; and, if we had not a satellite, might be easily explained by the atmospherical tides caused by the sun. But when we find that the barometer is not in the least observable degree affected by the moon's passage over the meridian, or by the united action of the sun and

moon at the syzygies, we have absolute proof that this cannot be the cause ; neither can the expansion of the mercury, being directly opposite to the phaenomena, the greatest degree of heat taking place at three o'clock, when the mercury is lowest.

With respect to the influence of the moon on the atmosphere, I was perfectly satisfied while in Beerboom that the cold season set in at the syzygies only ; and that there was always a considerable increase of cold at every return of them. But at the old powder works near Calcutta, I observed the greatest degree of cold to happen sometimes at the quadratures. Being, however, at that time much engaged in other pursuits, I did not attend to the circumstance of the moon's *absolute distance*, though of the utmost consequence in all calculations of the heights of the tide, to which the variations of the state of the atmosphere occasioned by the attraction of the sun and moon must be analogous. And yet this fact, important as it is to every seafaring person, especially in river navigations, as well as to ship-builders, for predicting the highest spring tides, seems to be totally unknown to the generality of these persons ; nor is it surprising, as it is not taken notice of in any treatise on navigation that I have met with. But M. de la Lande (Astronomy, vol. iii. p. 656) shews, that if the moon's mean force to raise the waters of the ocean be $2\frac{1}{2}$, her greatest force when *apogee* will be 3 ; and her least when *perigee* 2 ; a difference sufficient to account for the tides at the quadratures being sometimes nearly as high as those at the syzygies : a circumstance which was fully ascertained by a part of a committee instituted for examining plans for new powder works at the old fort ghaut, where stakes had been driven on purpose to find the rise of the tide. M. de la Lande confirms the theory by many observations made with great accuracy in some of the ports of France (Supplement, vol. iv.) and I can vouch for the fact by numerous measures of the heights of the tide both at the old and new pow-

der works. But you may easily satisfy yourself of the fact, by observing the height of a few tides at Champaul Gaut, when you will find invariably, that every great parallax of the moon, at the syzygies, is attended with a very high tide, and strong bore ; and *vice versa*. I have not been able to observe, that the moon's declination, notwithstanding what you may have heard from other quarters, has any perceptible effect on the tides.

I have been the more particular on this subject, that I have heard it made an unanswerable objection to your system, that the first attacks of intermittent fever do happen at the quadratures as well as the syzygies, and that relapses do likewise happen at the quadratures. Now should you meet with any such cases, the above observations may perhaps tend to reconcile them to your system, &c.

JOHN FARQUHAR.

Banky Bazar, 12th February, 1794.

No. 50.

I HAVE seen your letter of the 22d of June, and conceiving that it will be agreeable to you to know what opinion the late Doctor Daniel Campbell (who served in India till he arrived at the rank of Surgeon General, and practised medicine with great success) entertained on this subject, I sit down to inform you, that he observed and believed the influence of the moon in fevers. I remember particularly that he attended me at Calcutta in a very long and obstinate fever, which used to disappear and return again at certain intervals, without any deviations on my part from the rules that were prescribed to me. Upon my expressing my surprise at this circumstance, I recollect perfectly, that he told me that it was not at all wonderful, for that I was under the influence of the moon ; and that he had observed it all along. I afterwards attended to it myself, and found that the returns of my fever were connected with the periods of the full and change.

I shall be glad if the circumstance I have related can be of any use to you, being with great regard, &c.

J. HANNAY.

NO. 51.

A THOUSAND thanks for your instructive and kind communication of the 25th July. Of the influence of sun and moon in fevers, I think now as I have done ever since I became acquainted with your observations; and I was thence prompted to attend accurately to the subject. I have not the smallest doubt of the fact. I have seen it distinctly in very many instances in other people, and have had too much occasion to attend to it in my own case. And although from my inclination for natural history, and giving up much of my time and attention to that study, I have not kept regular journals of the different cases (which I now much regret), the reality of the influence of the heavenly bodies is so much fixed in my mind, that it enters into the consideration of every intermittent which comes under my care, as regularly as any other circumstance in the case, and I may say as necessarily. I am so much convinced of the existence of this influence, and of the importance of attending to it in the treatment of intermittent fevers in this country, that if you had not taken up the subject, and treated it in a manner at once scientific and elegant, I should certainly have thought it a duty to make public, what I consider not only as a curious fact in medicine and philosophy, but highly worthy of the attention of every person who has the care of the health of the community.

R. BRUCE.

Lucknow, 9th November, 1793.

NO. 52.

OWING to some mistake I did not receive your letter of the 22d June and 6th November till yesterday. This will, I trust, prove a sufficient apology for my not having replied to them much sooner.

To the many testimonies which you have received relative to the moon's influence in fevers, I can with truth add mine, having had many opportunities of remarking it during a residence of some years in this country. I have observed the paroxysms of fevers to have proved in general more violent about three days before and after the full and change, and to have gradually abated in violence after these periods: and I have found that convalescents from fevers, are more liable to suffer relapses at the full and change than during the intervals. Of this I have seen repeated instances.

I shall with pleasure comply with the request made in your second letter of the 22d November, as soon as opportunities occur.

C. COOTE.

Calcutta, 26th November, 1793.

No. 53.

I HAVE many apologies to make for not sooner acknowledging the receipt of your letter. When I was favoured with it, I had a severe inflammation of my eyes, and could not write without much pain. Upon getting well, I had totally forgot the honour you had done me, and was only recently reminded of it by a conversation in company.

I am sorry my experience and observations have not been extensive or accurate enough to furnish you with decided facts relative to the subject of your inquiry. The situation of Dacca, as you observe, is liable to intermittent fevers; but since I lived in it, few English residents have had regular agues: and those who have fallen under my inspection, have constantly confirmed your general doctrine by a very considerable accession of fever, about three days prior and posterior to the new and full moon. But although I cannot from my own experience affirm the extent of this influence to be equal to your correspondents, yet I must confess, I have seen several

local complaints very considerably affected by it. The time of delivery of women with child, as far as my limited experience goes, appears to be not much under the influence of these periods; but I have seen two or three instances of native women with rather severe intermittents, and with greatly increased paroxysms during those periods, who had no return of them after their delivery. There is a disorder prevalent among the natives in Dacca, and I am told very frequent to the eastward and southward of it, which begins with a swelling in the extremities, or in parts near the extremities. This complaint, called by the natives *sanjor*, is seldom accompanied with any fever in the intervals of those periods, but during them with excessive high fever and delirium.

GEO. DAVIDSON.

Dacca, 18th November, 1793.

NO. 54.

I SHALL lose no opportunity in taking the notes requested in your favour of the 6th instant, and it would afford me much satisfaction in being able to co-operate to the attainment of your purpose.

My not receiving your's of 22d June last, has prevented me from sending you a record of several cases of fevers, that from July till this present month occurred at this place; in every one of which the return of the paroxysm at the full and change have been particularly strong. In one case, it has been, after the first attack in September, so protracted and repeated with more or less violence at every change, that no later than Saturday last, 16th instant, another paroxysm returned. In a severe bilious intestinal fever, that attacked me last year about the end of July, at every revolution of the moon, and from that period, till I was fairly out of Bengal, having been so reduced as to seek the sea air, I had a return of the fever, though in a different shape.

That the influence of the moon is very visible in almost all febrile cases, I had ample opportunity of observing in my first voyage to this country in the year 1777, and subsequently till 1791, on board an Imperial East Indiaman. The ship being drove on a bank in the river Massuma, on the east coast of Africa, at the place called Behagoa Bay, the crew, through hard labour and unwholesomeness of the place, suffered much from an epidemical bilious fever, so as to have sixty and seventy patients ill with it at times out of 180, that formed the whole of the ship's company: almost every one having been seized in turn, several died.

On this occasion, as afterwards in the Gulf of Cambay, at the Nicobar islands, at Kedgeree, in the river Ganges, Houghly, and on our return, at St Jago, one of the Cape de Verd Islands, where the ships went and remained for a certain time in the course of the voyage, the crew was severely and repeatedly attacked by remittent and intermittent fevers. I observed, and was so fully persuaded of the effect of the moon's changes on them, that I took notice of it in a publication I made in my return to-Italy*, with a view of cautioning succeeding surgeons, coming from the Mediterranean in that country, that the method adopted in our practice there was by no means adapted to the treatment of the acute diseases that Europeans are liable to between the tropics.

NICOLAS FONTANA.

Barrypore, 20th November, 1793.

NO. 55.

I HAVE been favoured with your letter of the 25th of July and 12th instant, and though I have had much opportunity within that period of observing the sol-lunar influence on fevers, &c. I cannot say that I have met with any thing here

* The publication is "Osservazioni Sopra le Maffattie che attaccano li Eftropoi nei climi Caldi, &c. Livorno, 1781."

to determine me in its favour. Since Mr Stephens left this place, I have had the charge of the native hospital, and though I have constantly had from 100 to 150 sick, and those mostly fevers and fluxes, and have paid pretty strict attention daily to the cases, I have found, in general, that the paroxysms have been less violent about the full and change, than during the intervals between those periods. Our hilly situation here, I thought, very favourable to determine this point, as we have had many agues and fixed intermittents; yet, by every observation I have been able to make, I think it has rather been against the lunar system.

Mr Stephens promised to answer your letter on this subject, on his leaving this; if he did not do it, I trust you may have met with him in Calcutta, as I conceive his observations here will corroborate what I have said *.

J. W. BAINBRIDGE.

Chittagong, 21st November, 1793.

No. 56.

Two very bad cases of fevers have lately fallen under my care, &c. &c.—The patient of the 2d case is at this moment in extreme danger, and, from the state of the moon, I am under the most serious alarm for him.

* I saw Mr Stephens as he passed Calcutta on his way to his present station; but his information was by no means against the lunar system. He allowed that the relapses of fevers were generally connected with the full and change of the moon, which we shall afterwards shew is an observation comprehending in itself alone sufficient proof of the general law.

As Mr Bainbridge's letter differed widely both in matter and inference, from all that I had received, I applied to him immediately on the 5th Dec. 1793 for some particular information. However, although he has acknowledged my letter, I have as yet been favoured with no reply to it.

FRANCIS BALFOUR.

Calcutta, 20th February, 1794.

I make no doubt, nor does any medical man, I presume, of the utility of your researches, and also that the faculty in general, and the world at large, will benefit by them. It is astonishing that those laws of nature which so clearly shew themselves, should have been so long neglected and unexplored by so many writers of science, in our own neighbouring nations. I sincerely hope you will meet with that support you merit, and that the just tribute of well earned applause may prove satisfactory to your feelings, for the labour you have subjected yourself to.

JAMES LAIRD.

Dacca, 15th November, 1793.

NO. 57.

IN the province of Rungpore, where I have resided upwards of eleven years, from my own observation, the general law is indisputably established; and I have to lament, that cases cannot be produced to convince others, as well as I am myself, of the lunar influence in fevers and agues, the most prevalent diseases in this part of the country. However, I do not despair of collecting a few cases previous to my departure, which I shall take great pleasure in communicating to you, arranged in the manner you require. I trust, my good sir, you will pardon my remissness, &c.

ROBERT COLLINS.

Rungpore, 19th December, 1793.

NO. 58.

I BEG leave to enclose you a case of abdominal obstructions, which occurred to me since I was favoured with yours of the 22d June, and wherein I paid every attention in my power at the full and change of the moon.

CASE.

I was consulted by a lady about the middle of August last,

who had been afflicted with abdominal obstructions for upwards of twelve months. The symptoms were as follows : "as soon as she awoke in the morning, she was seized with anxiety and a very unusual and disagreeable sensation a little below the umbilical region, attended with very little pain.

" From the time she arose in the morning till twelve at noon, she had very loose motions. The remainder of the day and night she was tolerably well ; except being seized, after sitting for a length of time, with a swimming in the head and great faintness."

I commenced giving her small doses of calomel and opium. From the time she began with the above medicine till the case was completed (the first of November) she never once escaped the effect of the moon : which always took place three days before the full moon. At these periods all the symptoms returned with great violence, and continued for two days ; but as her complaint gave way to the medicine, the influence of the moon gradually became less violent.

I made particular enquiry if she had paid any attention at the full or change of the moon before I saw her. She says she very well recollects, that every thirteen or fourteen days she was much worse, just as I have seen her when she first applied to me ; but as to the time of the moon she could not remember, as she had no idea that it could have any influence on her disorder : however, I have not the least doubt that it was about the same time I have observed during my attendance.

With respect to the influence of the moon on fevers, I have so repeatedly observed it, both with Europeans and natives, that in my opinion it is beyond all doubt : however, I do not mean to say that the paroxysm always occurs at the same time

of the moon, as in the above case, but often at and after the full and change.

P. EWART.

Barrackpore, 18th January, 1794.

NO. 59.

I HAVE perused your letter, and am fully persuaded, from experience, that the moon has a very powerful effect on fevers.—After a fever which I caught at Ramghur, I suffered relapses almost constantly at the full and change of the moon for four or five years; and could, with confidence, during that time, announce the arrival of the springs without consulting an almanac.

I forwarded your letter to the gentlemen of my battalion, and from them I have no doubt you have obtained much information: for almost all of them suffered from fevers, and were subject to similar relapses at the full and change.

ROB. DENNIS.

20th January, 1794.

NO. 60.

I HAVE seen your letter of the 22d of last June, and from the obvious utility of the investigation in which you are engaged, I feel myself called upon to let you know, that I have long observed the influence of the moon in feverish complaints; but more particularly at Pettebeat, in the Rohilcand country, where I resided during the rainy season of the year 1779. Most of my servants, and indeed a great many of the inhabitants of that town, which is situated near the forest skirting the hills, were attacked with severe intermittents; which I understood was the case every season, and were sometimes very fatal.

As I had no surgeon with me, I was obliged to treat them

myself: and was so successful in removing the distemper, by first premising plentiful evacuations by tartar emetic, and then giving the bark abundantly during the intermissions of the fever, that I had soon a great many patients from amongst the better sort of people of the town, who requested I would give them medicines; and I really cured a great many of these intermitting fevers. During this period, I observed, that the returns of the fever were generally about the full and change of the moon; and long before I had heard the matter agitated, in consequence of the opinions you had started, I had formed an opinion, that the fluids of the human body, as well as the tides and atmosphere, were much affected by the situation of the moon: and have now a conviction, from a continued observation, that there is not a distemper of any kind that is not, as well as that which by preference is termed lunacy, much influenced by the situation of that planet.

ALEX. KYDD.

25th January, 1794.

NO. 61.

TEN thousand pardons for the long delay. I intended descending on the lunar influence at considerable length, and deferred it to a moment of leisure; which, however, I have not enjoyed, and am not likely to enjoy for a great length of time. I shall therefore give you the testimony of the people here in two or three words.

In favour of the lunar influence, Lieutenant Dick's testimony is the most pointed. I send you his note enclosed. Captain Welch declared, that having formerly been affected with what is called a hill fever, he continued for many years to perceive its effects every full and change of the moon, even during his residence in Europe; and that he was always liable to febrile symptoms at those periods, until his arrival at Assam.

The officers of the detachment serving in Assam at pre-

sent, are unanimous in their declarations, that they have either experienced this influence themselves, or have obtained a conviction of its reality from the experience of their brother officers.

With respect to my own experience, I have not considered the results sufficiently as yet to authorize me to entertain a decided opinion on the subject. I believe, however, that in all febrile cases of neglect or mal-treatment, those symptoms are liable to recur at the lunar periods. Old complaints of every kind, when attended with febrile symptoms, are, under similar circumstances, exposed to the operation of the same influence; which on all occasions would appear to have a more obvious effect on Europeans than on the natives. In cases where the patients have not been neglected or mal-treated, I have seldom perceived any decisive proofs of the existence of this influence.

In this hospital I have sometimes imagined that I perceived its operation; but the effects were not sufficiently obvious to produce conviction, unless in one instance of an European officer. Upon the whole I think the coincidence of fevers or relapses, in a variety of diseases with the principal lunar periods, is an established fact: although the extent of this coincidence will for ever prove a subject of doubt and of dispute.

The admissions into the hospital have been	665
The discharges	595
The deaths	33

OF THE DEATHS.

Refused or took no medicine	23
Brought from the jungles 10 or 15 days ill of fever before admission	5
Old neglected venereal	2
Of the fever and fluxes	3
Total of fevers	303

Deaths of fevers exclusive of the old cases and who refused medicine 3

J. P. WADE.

Assam, 15th January, 1794.

DEAR DICK,

Oblige me by putting down on paper what you have experienced yourself of the moon's influence, and what you know others have experienced, and send me the result to be transmitted to Balfour.

J. P. WADE.

DEAR WADE,

My observations were not so minute as to enable me to give any satisfactory information regarding the exact time of the day of the attacks of the fevers, or the violence or abatement of the paroxysms. But I am very confident that most of the sufferers who were afflicted with it, used to experience a relapse at the full and change of the moon; and that this influence is still felt by them at this distant period.

G. DICK.

No. 62.

MANY of the natives at Guttal and Radanagore, are afflicted with swellings in their legs, similar to what you have described, but I have not observed that the weavers are more subject to it than other classes of labouring men.

They certainly complain most on the springs, both at full and change, at which times the pains are more acute, accompanied by fever, which subsides as the springs go off. Some are afflicted in both legs, though most commonly I think only one leg is swelled. It is a grievous disease, and if you can point out any medicine likely to remove it, you will much oblige me: or if you wish to make more accurate observations

than I have made, or am capable of making, both on the nature of the disorder and its probable remedy, I can send 3 or 4 men afflicted therewith to you at the gardens ; where, in hopes of a cure I am convinced, they will be happy to attend.

P. TOUCHET.

Chouringhy, 25th January, 1794.

No. 63.

I AM sorry that, partly from hurry of business, and partly from negligence, I have not kept so regular a journal since I came to Calcutta as to enable me to give a decided opinion on the subject of sol-lunar influence.

For several years past I did indeed keep a journal during the months of July, August, September, and October, when fevers were most prevalent ; and observed, that attacks were more frequent about the change and full, than during the intervals between these periods.—I also observed, that the accession of fever was most common about mid-day ; but cannot with certainty declare, that the paroxysms in every stage of fever shew themselves in a greater degree of violence about the change and full : because the medicines that were given must have disturbed the regular course of the paroxysms, and made it difficult to ascertain with exactness what effect the moon's influence had on the fever.

There is nothing, however, of which I am more convinced, than that relapses are most frequent during the periods you mention :—for I have sometimes had patients in remittents and intermittents during the rainy season, who afterwards had regular returns every change and full of the moon, till the months of February or March.

I have observed, that fevers which are symptomatic of obstructions in the liver and spleen, especially the latter, and also

hectic fevers, have been more intense at the full and change, than at any other period. The influence of the moon was easily perceived in these fevers, as no bark could be given.

Patients ill of other disorders have often assured me, that their complaints were always worse at the full and change. But as almost every body in Bengal is prepossessed with an idea, that the moon has a baneful influence; and as in chronic diseases I could only have the patients' word for it, I generally thought that they were unintentionally deceiving themselves and me. I have only further to add, that sol-lunar influence appears to me to be most powerful from the first of July till the first of January, &c.

WILLIAM DICK.

February, 8th, 1794.

NO. 64.

EVER since I received your letter, I have been so unwell, that I was unable to answer it as I wished. Although I am now better, you must still be satisfied with the result of my observations in intermittent or remittent fevers, without requiring a detail of the cases or peculiar symptoms. In these fevers I have seen so many instances of relapses occurring regularly every full and change of the moon, for many months together, that I cannot for a moment doubt that the one depended on the other: and in these cases I found bark and other medicines much more effectual in preventing the attack, when given immediately before the commencement of the lunar influence and during the first days of it, than when given at any other period. As my observations in other diseases have neither been so numerous nor conclusive as the foregoing, I will not trouble you with them at present, &c.

JAMES HARE.

Calcutta, 9th February, 1794.

No. 65.

I HAVE to acknowledge the receipt of both your letters, which various interruptions, but particularly the general relief of the troops, and the necessary preparations for moving from my former station to Calcutta, have prevented me from answering so soon as I wished.

The General Hospital of the Presidency has now come under my charge. From the number of patients constantly in it, and the mode in which the hospital books are now kept, I hope to be able to lay before you, in the course of a year, a great number of cases that may not be unworthy of your acceptance. In the mean time, although I have not been able to ascertain how far the first attack of fever is induced or affected by sol-lunar influence, yet I do not hesitate to assure you, that both in Bengal, and on the other side of India, where I served with General Goddard's detachment, I have observed in patients greatly weakened by fever, in general, the relapses and exacerbations of the whole tribe of febrile affections, so much connected with the periods of the full and change of the moon, and their remissions and abatements so much connected with the intervals between these periods, that I do not entertain a doubt of sol-lunar influence. May you long enjoy good health, my dear Balfour, to enable you to pursue your studies for the good of mankind.

G. BOYD.

Calcutta, 30th January, 1794.

No. 66.

Part of NO. 4, omitted by mistake in its proper place.

IN 1790 I well recollect that my fever used to return at new and full moon, regularly. The coming in of the *Bore* *, which

* The tide which runs up the river like a very high surf, with great violence and noise, during the springs.

we heard from a great distance, used to be a sure sign to me of the fit coming on. I do not believe I had any cold aguish fits at Ghyretty; nor have I any recollection whether the fits were most severe in the day or the night. In 1780 my recollection is somewhat better, but it does not reach the points you require to know; viz. I do not recollect the particular time of the day or night when the fits came on, or whether they were more severe in the one than the other. I recollect with horror what I felt, and can positively assert, that the new and full moon, for near eight months together, used, to the best of my judgment, to influence my fever. I had regularly five attacks, the third one the strongest, and each attack, to the best of my recollection, was later than the former nearly three quarters of an hour. Permit me to add, that I have invariably observed, (for years) myself affected with symptoms similar, though in a small degree, about the full and change, compared to those I felt in 1780: and frequently have answered my friends, who observed a change in my looks, (till within these five years) that the cursed moon was sitting on my head. I only observe this trivial matter as a proof that I ever did, and do now, believe the moon had an influence in my violent fevers of 1770 and 1780.

The little information I have given you on this subject is, nevertheless, my best. Others I have not been able to prevail upon to write, though they acknowledge having felt the same.

JAMES PRINGLE.

Calcutta, 12th October, 1793.

NO. 67.

*This number contains the Observations of Dr James Lind,
in his Thesis.*

Vis Solis ac Lunae ad Bengalam mirabiliter ad recidivam hunc morbum perpressos proclives efficit; unde hanc inter

causas excitantes morbi annumerare possumus ; est enim ita insignis, ut aegrotus, qui octo aut decem diebus convaluit, in summum recidivae adductus fuerit periculum, antequam lucidum coeli decus plenum orbem impleat, aut sub interlunium, ni cortex Peruvianus inhibendi causa detur. Documenta tam multa sunt, ac res ipsa ita cunctis Bengalae degentibus innotescit, ut modo dixisse sufficiat *.

Quosdam ipse vidi, qui non nisi sub ipso plenilunio et interlunio paroxysmum, et tum unicum tantum, habuere ; qui ta-

* Decimo sexto calendas Novembris, anno 1762, tempore pomeridiano, solem hnd parum lumine defectum vidimus ; ac cum antea sub serena coeli facie ex nimio aestu laborabamus, mox inter maximam obscuracionem aer multum algebant. Tunc temporis, remittens febris qua tenebatur D. MACQUIRE a praefecto in concilio tertius subeunte solis defectu, exacerbata est ; eodem vero transeunte, evanuit. Aegroti vero qui tum nostra sub cura erant non admodum affliciebantur.

Subitos ac violentos lunae effectus tum praecique observavimus 4to Nonas Novembris, hora circiter secunda matutina, quo tempore terra interposita radios intercepti solares ; in eo temporis articulo haud pauciores octo nautarum ex febribus convalescebant, eodem fere temporis puncto, vehementissimo paroxysmo sunt correpti ; et idem plurimis evenit qui in nave fuere collegae nostri curae demandati.

Quicumque hanc rem ad praxin medicam adhibere velit, studium omne suum conferre debet in hisce observandis quae in hac regione a luna perficiuntur. Huius motus, et aestuum ex his pendentium theoriae, idea generalis ex operibus D. JACOBI FERGUSON, R. S. S. optime parari potest : Hisce igitur perspectis, arti medendi addicti facile cognoscere possunt, quo potissimum tempore vis lunaris maxima sit, et qua praecique tempestate effectus ejus praecavendi sint. Nos quidem nauticam illam ephemerida commendamus publici juris a Reverendo D. Maskelyne factam in usum eorum qui in patenti oceano longitudinem explorare velint, quaeque motus omnes lunares exacte persequitur. Intra Tropicos lumina coelestia saepe verticalia sunt, ac proinde majori vi agunt quam in regionibus altiorum ut vocant latitudinum : Literatissimus autem MEAD complura collegit exempla virum solis et lunae sub coelo Britannico ; at in India Orientali et Occidentali, ut in quavis alia intra tropicos regione, multo plura et notabiliora facile colligi possunt.—Vide page 25. &c.

men facili negotio arceri potuit, si cortex peruv. paucos dies ante expectatum accessionis tempus exhibitus, ac usque quo illud sit elapsum continuatus fuerit, in hujusemodi easa febricitanti puero cuidam Mauro ipse sanantes porrigebam manus, morbum usque ad quatuor paroxysmus quovis plenilunio et interlunio tutò reduentes permisi, ut prius rem ipsam exploratam haberem, quam cortieem coercentem admoverem.

No. 68.

IN room of the short extract of Dr Jackson's paper, taken from the European Magazines, I have now the pleasure to insert the original paper at full length. I am sorry however to observe that his account of my first Treatise *is so very inaccurate*, that to prevent the misconceptions that must naturally arise from it, I shall be under the necessity of pointing it in the APPENDIX, which I mean to annex to this volume; with *similar strictures* on his Treatise on the fevers of Jamaica. To render a reference to the passages of this paper, on which I shall have occasion to remark, more convenient, I have taken the liberty of printing them in *italics*.

Extracted from the London medical Journal Vol. VIII. Part I. octavo for the year 1787.

II. *Some Observations on the Connection of the new and full Moon with the Invasion and Relapse of Fevers. By Robert Jackson, M. D. Physician at Stockton, in the County of Durham, communicated in a Letter to Sir Joseph Banks, Bart. P. R. S. and by him to Dr Simmons.*

To Sir JOSEPH BANKS, Bart. P. R. S.

SIR,

THERE is a subject which at present seems, in some degree, to engage the attention of the Public—I mean the influence, or, to speak more circumspectly, the connection of the new and full moon with the invasion and relapse of fevers. *It is now about a dozen years since I made some observations on*

this matter in the West Indies, and probably I should have made them known to the world before now, had I been able to have carried them on in the extensive manner I originally intended ; but the necessity of attending to some concerns of life has obliged me to leave the design, if not totally, in a great measure unfinished. In the West Indies, indeed, in the island of Jamaica, I think I may say my observations were made with care. The same care was continued in the southern provinces of America ; but my papers, with original remarks, having been lost in one of our unfortunate rencounters with the enemy, I can only charge my memory at this distance of time with what was the general result of them. In France, Germany, and Italy, I had made a few cursory remarks ; not numerous, or pointed enough, however, to be depended on. In England I can say nothing, my opportunities of observing here having hitherto been very confined.

Within these two years the world has been favoured with a treatise on this subject by Dr Balfour, a gentleman who has resided and practised several years in India : but his account differing materially from what was observed by me in the West Indies and in America, I take the liberty, Sir, of presenting to you the result of my inquiries on this subject, without any other apology than what the importance of the subject carries along with it. It is a subject which I am persuaded will be curious to you as a philosopher ; and the knowledge of it may be of use to the physician. This, then, being the case, I shall beg leave to mention to you in a few words, and a very few words will suffice, how the idea arose, and the manner in which it was prosecuted.

I went out to the West Indies in the beginning of the year 1774. I was apprized of what Dr Lind had mentioned, as an effect of the new and full moon, and of eclipses, on the relapse of fevers in India, and might be supposed not unprepared to expect something of a similar kind in Jamaica. Accordingly, before the end of the year, I had reason to believe it was a

thing even in that country not without foundation. In the course of the year following I went farther—I observed that frequently three or four of a company of soldiers, who were quartered at the place where I resided, and of whom I had the care, fell ill on the same day ; and that this did not happen, perhaps, above once in a fortnight. The thing seemed curious; and as it happened repeatedly at the time the moon was near full, a hint was suggested, that not only the relapse, but that the first attack of fever was probably connected with the changes of that planet. To ascertain the truth of it, however, in the beginning of the year 1776 I provided myself with an almanack, and on the blank leaves of it marked the precise date of attack of such fevers as came under my care. At the end of the year, of thirty cases of proper remitting fever, I found that twenty-eight had happened on one or other of the seven days preceding a new or full moon. The year following, of twenty-eight, there were only twenty-two: it was remarked, however, that of those six which happened in the period which follows new and full moon, three had happened on the day of the moon, and only a few hours after the change had taken place. Besides those cases of proper fever, I had taken notice of a number of day fevers and slight feverish disorders, the attack of which seemed likewise to have been influenced by the same cause, though in a smaller degree.

This precisely is the case as it stood in the almanacks, accompanied, however, with a few remarks and explanations; the principal of which are the following :—That though the seven days preceding new and full moon, or the second and last quarters, are what might be reckoned the sickly period, yet it was in the four days immediately preceding that the attack of fevers was chiefly remarkable; that the invasion of fevers was always nearer new or full moon in the dry and healthy season, than in the rainy months, when diseases were common, particularly when they were epidemic; that this was likewise more observable in the mild, remitting fevers, than in

those of a violent and malignant kind ; and in the soldiers of the garrison, who were exposed to few occasions of disease, than in the inhabitants of the town and country, whose various employments subjected them oftener to fatigues, or carried them oftener to unhealthy situations.

The above observations, which were made at Savannah la Mar, in Jamaica, seeming to carry with them a very decided proof of the influence, or connection of the moon with the invasion and relapse of fevers, I thought it might be of consequence to prosecute the idea in the different countries I might happen to visit. In the year 1778, I joined the army in America, and went to serve in a regiment that was quartered on York island. In the months of June and July, the regiment being encamped in a dry and healthy situation, fevers were rare ; and such as happened, were in the period approaching to a new or full moon. In August, it was removed to Kingsbridge, and encamped in the neighbourhood of some low and marshy ground, where it continued the whole of the autumn. An intermitting fever soon began to appear, and soon became highly epidemic : its time of invasion, with respect to the moon, was greatly more irregular than it had been in the former months ; at the end of the year, however, of one hundred cases, about eighty were found to be in the period above mentioned. What seemed remarkable, relapses were rather in a smaller proportion. In the years 1779, 1780, and 1781, the regiment served in the southern provinces, and may be said, indeed, to have been almost constantly in the field : it was often encamped in unhealthy situations, and often had the intermitting fever epidemic in a high degree. When that was the case, as was observed before, the irregularity of invasion was greater ; yet even then, the approach to new and full moon seldom failed to double the number of the sick : but my memorandums having been lost, I cannot exactly ascertain the proportion in the three last campaigns.

Having related the above observations, which I flatter myself were made in a manner little liable to deception, and in the noting of which I was not consciously biassed by theory, it may not be amiss to take a view of the account given by Dr Balfour. The three days immediately preceding, and the three days immediately following, new and full moon, are what are mentioned by him, as the period remarkable for the invasion and relapse of fevers: but it is pity, he does not tell us on what facts he founded this opinion. If it is drawn from a loose, from a gross estimate of what he thought he saw, it cannot be depended on. Unless a man is circumstantial in his facts, and very circumstantial too, a preconceived opinion leads him astray; for it is seldom, very seldom, that theory does not run before observation. What he observes, indeed, it must be allowed, is more agreeable to what we suppose to be the effect of the moon on the tides: but the case here seems widely different; and, probably, were we to reason on a subject that ought not, indeed, to be made the subject of reasoning, we might find some explanation of it. It being agreed on both sides, that the new and full moon are to be considered as a powerful exciting cause of fever, it is more reasonable to suppose, that that cause will produce its effect while it is acquiring vigour, rather than while it is losing it; that is, in the days that precede the new and full moon than in those that follow: but this is only a supposition of what may be the cause; the fact is certain, as far as a fact in physic can well be. And were any more proofs wanted, to the above we might add the different periods at which fevers have a tendency to relapse. In the West Indies, and in America, particularly in the West Indies, where the crisis for the most part was the work of nature, fevers, when they did return, almost always returned on a seventh, a fourteenth, a twenty-first, or a twenty-eighth day from the termination of the fever; more of them, indeed, on the fourteenth than on all the others put together. This observation was not unknown to many practitioners in Jamaica; it was common with them to say, when a person had a bad fever, that he would either have a

return of it in a fortnight, or some other disease ; and the prediction in the course of three or four years was seen seldom to fail.

The above facts having put it beyond a doubt, that the new and full moon, or the approach to new and full moon, is a powerful exciting cause of fever, it would be a matter of much consequence to determine the degree of it in the different parts of the world. Surgeons of regiments, and those who have the care of convents and hospitals, have the best opportunity of coming at the truth. Observations made on the people at large, who live in a thousand different ways, will always be uncertain ; and the accounts that physicians receive of diseases that they do not see till a late period, are not at all times to be depended on. *The importance of the subject is such, that it well deserves to be inquired into. It particularly concerns the army ; and it is no rash assertion to say, that a knowledge of this principle, and a knowledge of the proper use of bark, will go farther in preserving the health of an army, an army on service, than all the other helps of medicine put together. Of this I had a strong proof during the campaigns of 1780 and 1781, in the corps I had the honour to serve in.*

I am,

With great respect,

SIR,

Your most obedient

Humble servant

ROBERT JACKSON.

Stockton, Dec. 3, 1786.

NO. 69.

Observations from Dr Blanc, Dr Moseley, and Dr Hunter.

ALTHOUGH none of these gentlemen seem to have paid any

attention to sol-lunar influence, yet each of them has contributed a little to shew that it exerts its power in the region of AMERICA as well as of ASIA.

Dr Blane mentions the case of a physician of some eminence in the West Indies, who always enjoyed uninterrupted health, which he imputed to his taking from half an ounce of bark every change and full of the moon, as he thought that fevers of the remitting and intermitting kind were more apt to recur at these periods. *Vide Obs. on the Diseases of Seamen, &c. p. 225.*

Dr Mosely gives a case of pulmonary hæmorrhage, in which the effusions of blood obeyed the periodical changes of the moon. *Vide Treatise on Tropical Diseases, at the end.*

Dr Hunter, in his accurate account of the periodical changes which took place in the West Indies in the state of sores and ulcers, has described a chain of phaenomena which is similar to what we have seen in India connected with the revolutions of the moon; and which we cannot, therefore, reasonably refer to any cause but sol-lunar influence. *Vide Obs. on the Diseases of the Army in Jamaica, Chap. VI.*

✂ The historical parts of the preceding Treatises which were originally inserted under NO. 70, are now omitted, as they can easily be referred to, in the Treatises themselves.

NO. 70.

To these extracts, I am happy to have it in my power to add the following paragraph from a letter which I received from Dr Davidson, of Kinross, in Scotland, after the publication of my second Treatise.

I delayed acknowledging the receipt of your's, in hopes I might have been able to determine whether your theory ap-

plies as well to this country as to India. It appears to me plain, that even in this variable climate, where sol-lunar influence might be supposed less constant, that the paroxysms of fevers are always severest at the full and change of the moon. My attention being but lately called to this subject, my observations have neither been numerous nor distinct; but I must suppose, from what I have observed, that the lunar influence is in this country great, &c. &c.*

NO. 71.

NOTWITHSTANDING all our attention to the phaenomena of fevers, a circumstance in the history of their paroxysms, which, united with the laws that we have already described, enables us to explain all the forms and varieties that occur in nature, and constitutes one of the most interesting traits of their character, had escaped our notice, and was not till lately discovered.

I mean “*their TENDENCY to postpone the hour of their attack at each succeeding return.*”

The existence of this tendency is now assumed on the foundation of the two following circumstances:

1. *That when fevers are properly treated, their paroxysms very frequently postpone in the course of the springs; and*

2. *That when fevers are properly treated, their paroxysms almost invariably postpone in the course of the neaps.*

* Upon the whole I think it probable, that the observations of the ancients, made in EUROPE many centuries ago, and those made in later times by Ballonius, Ramazzini, Mead, Grainger, and others, may in time be reviewed with less prejudice; and that the philosophers and physicians of that quarter of the globe may yet be inclined to believe, that the action of sol-lunar influence in fevers is not entirely confined to ASIA, AFRICA, and AMERICA. Vide Dr Jackson's Treatise on the Fevers of Jamaica, in the Notes.

The first of these circumstances is a phaenomenon which I am conscious of having observed so often, before I discovered that it led to establish any general law, that I conceive it cannot fail to be familiar to every physician of experience ; and the observations which I have since made with more attention, and the facts stated in the preceding letters, with the information I have obtained from others, confirm me in this conclusion.

But this tendency in the paroxysms of fevers to postpone, does not in every case shew itself steadily until the commencement of the neaps, when, becoming very soon after the transition from the springs entirely nocturnal, they then evidently, and I may say invariably, postpone the hour of attack later and later every succeeding night ; and fall even at last within the limits of the morning hours between 4 and 9 o'clock ; during which it is very uncommon for paroxysms to come on in the springs.

It is true that the paroxysms are not unfrequently observed to *anticipate*. But this we are generally able to ascribe to some occasional increase in the disposition to paroxysm produced by improper management, or some other obvious occasional cause : for when fevers are properly treated, when the bowels are kept sufficiently open, and the patient is managed in other respects with judgment and attention, the natural and constant tendency of paroxysms, like the tides of the sea, is to postpone their attacks at each succeeding return. *And as the paroxysms of fevers which happen in the nocturnal hours (i. e. between 9 P. M. and 4 A. M.) during the neaps, postponing a certain space every night, will often at length appear at an early hour in the morning, we must be cautious not to consider this transition from night to morning as an anticipation, which is in reality effected by a regular succession of different postponings ; and which, occurring in the course of night, are apt to escape the notice of a person who does not prosecute his observations with a fixed and unremitting attention both night and day.*

No. 72.

Extract of a CEYLON letter from Thomas Christic, Esq. superintendant of Hospitals on Ceylon, to William Hunter, Esq. Marine Surgeon in Bengal; dated Columbo, 8th Decr. 1803.

Although I had attended to Dr Balfour's work, the solar influence was so little to be remarked here, that I confess I was an unbeliever in that principle, till the existence of the prevailing fever; contracted in the Candian territory; when its influence became very evident in many cases.

I have for two months kept a pretty regular register of the Barometer, and am quite convinced that the diurnal variations, pointed out by Dr Balfour, extend to this Island; but I have not been successful in tracing any connection between these variations, and the daily revolutions of fever.

A true Extract

WM. HUNTER.

SUMATRA.

On my first arrival I had a severe fever with obstructions, which however I soon got rid of, and, thank God, my state of health is now pretty good. The fevers here are alarming and rapid in their progress—We overcome them by mercury, and antimony combined, arsenic, and particularly the bark.—I have seldom found your practice fail in these fevers if vigorously pursued. The lunar influence is generally allowed here, and we have I'm sorry to say, too many proofs of its painful agency. I am at present engaged in the prosecution of some experiments on this subject, the result of which I shall communicate to you when completed.

JAMES LUMSDAINE.

Fort Malbro, 6th Sept. 1798.

PANANG.

It gives me much satisfaction to be able to add mine to the numerous testimonies you have already received, in confirmation of the great effects of lunar influence in many of the diseases incident to these climates, a doctrine which you have very ably illustrated, and in my opinion established by the sure test of facts on the surest foundation. I confess I was a little sceptical on that head, till I read your excellent treatise on the subject, which soon dispelled my doubts, and convinced me of the truth of the doctrine you have so clearly laid down.

I have been at some pains, during a considerable practice of some years, at Prince of Wales Island, in observing the effects of the moon in the prevailing diseases of that place, of which at present I can only give you the general result.

In the diseases peculiar to that island, as fevers of the intermitting and remitting kind, dysenteries, diarrhoeas, liver complaints, rheumatic affections, &c. I have generally found the violence of the symptoms considerably increased during the full and change, and relapses particularly frequent at the same periods; which I attribute to the same cause. By keeping that in view, I have been enabled to administer the medicines adapted to the different diseases, with a greater degree of certainty and precision. Wishing you a successful termination of your literary labours, in which the advancement of medical knowledge, and the interest of humanity are so deeply interested,

I am, dear Sir,

Yours very sincerely,

J. HUTTON.

Calcutta, February 28th, 1803.

No. 73.

Respecting the parallaxes.—From Mr Howison, 1792.

I received your note on my return home yesterday evening, accompanied with the Ephemeris for this year.

So remarkably have I been disappointed in the usual remissions since the commencement of the present neaps, that it was my intention to have noticed it to you on our first meeting, being perfectly ignorant of the same circumstance having attracted your observation.

Out of 15 patients under my care with intermittents, not one escaped exacerbations on the 1st and 2d instant, and of longer duration than ever during the last springs, which in some measure staggered my lunar faith, being unacquainted with the cause to which you seem justly to ascribe them. Yesterday the quotidian paroxysms less violent, and to day with many complete intermissions.

I have given your note to Drs Campbell and Fullarton, they know nothing of my remarks; let us see how we correspond.

The Ephemeris shall be returned in safety. I am happy to hear Campbell is in so fair a way; should have called had I not been much engaged with my saltpetre manufacture.

I remain yours truly,
J: HOWISON.

Calcutta General Hospital, 4th Dec. 1802.

Respecting the parallaxes.—From Dr Fullarton, 1792.

In consequence of your request, which Mr Howison communicated to me yesterday, I have carefully perused my hospital journals, and think I cannot better answer your queries, than by sending you an abstract of the present state of all the cases of fever and dysentery (without exception) in my ward,

so far as they are connected with the lunar periods. You will find from the statement, that out of 16 patients, 5 only have had any remissions during the late neaps, and that in 2 even of these 5, the remissions were rather obscure; that 5 have had their fits protracted without abatement, during the neaps, or at least till the 4th of December, when the moon's parallax began again to decrease; and that no less than 6 were attacked during the neaps, for the first time. This coincides with what you have remarked in Dr Campbell's case, who, I am sorry should be the victim of the untoward circumstances of the heavenly bodies. Should you wish for any further information or observations relating to the cases here, I shall be happy to give you all the satisfaction in my power.

I am,

Dear Sir,

Your very humble servant,
JOHN FULLARTON.

General Hospital, 6th December, 1802.

1. *Cases which remitted on the approach of the late neaps.*

- 1 Steele.—Admitted November 30th, with quotidian of 5 days duration, which was stopped by the usual evacuations.
- 2 Woodward.—Attacked with quotidian in the beginning of the neaps, before last; lingered on in an uncertain state during all the succeeding springs; and had a perfect crisis at the commencement of the late neaps.
- 3 Collins.—Attacked with remittent fever at the commencement of the last springs, which yielded on the approach of the neaps, and has not recurred.
- 4 Gibson.—Has slight lingering fever, with obscure paroxysms, and pains in his limbs and bowels. Had a remission on the approach of the late neaps; but since that, has made no progress in recovery.

- 5 Spooner.—Admitted on the 28th November, with dysentery of 5 weeks duration. He was relieved in the neaps by the usual medicines, but is not yet cured.

2. Cases which continued during the neaps unabated.

- 1 Vickmann 1.—Came into the hospitals with slight fever and dysentery, which underwent a severe exacerbation on 26th November, (5th day of the springs,) and continued without appearance of remission through the neaps, till the 4th December, when some slight abatement took place.
- 2 Davis.—Has been lingering in the hospital since November 15th, with chronic dysentery, torpor, and slight fever. No appearance of remission till the 4th, and then partial.
- 3 Newton.—Attacked with quotidian November 27th. The paroxysms continued with equal violence till the 4th, and have not yet ceased.
- 4 Muekleston.—Attacked with dysentery and fever at the commencement of the neaps before last. They became worse at the approach of the springs, and have not abated materially during the late neaps.
- 5 Freeman.—Admitted 30th November, with tertian fever of a month's standing. No remission during the neaps.

3. Cases which suffered the first attacks during the late neaps.

- 1 Adams.—Attacked with quotidian on December 2d. No abatement till this day, (6th December.)
- 2 Dawes.—Attacked with quotidian November 29th, which has continued daily during the neaps with little or no abatement, notwithstanding the usual medicines.
- 3 Buckley.—Attacked with remittent fever, December 2d. Symptoms unabated, and very severe.

- 4 Stillman.—Attacked with quotidian, December 2d. No abatement.
- 5 Blake.—Attacked with a paroxysm of fever on November 29th, which recurred on the second succeeding tertian periods, but missed him on the 5th.
- 6 Fowler.—Admitted with catarrh on November 29th, and was seized with a fit of fever on December 2d, but it has not since recurred.

J. F.

6th December, 1802.

Respecting the parallaxes—from Dr John Campbell, 1792.

THE unusual number of fever patients—26—under my charge, these two last months, has given me a fair opportunity to observe the effects proposed in your queries.

Fifteen patients were admitted from November 26th to December 1st. Five cases suffered no alteration during their course. Two were relieved from the 5th to the 9th, and relapsed, and seven ceased on the 4th, one the day following, and are convalescent. Eleven were older patients.—Five convalescents had relapses on the 30th -1st and -2d and three of these recurred on the fourth, one on the 3d, the other on the 5th. Three were salivated before the 24th November, and had an exacerbation of their Ptyalism, for several days after the 30th.

Of the total 26—*Ten* seem to have followed the encrease and decrease of the horizontal parallax, *Seven* affected by it, and *Nine* in which I could observe no connection.

The fevers are intermittent, remittent, and continued, but pass and repass into one another.

I have an old venereal, rheumatic patient, Alexander Nixon,

who has been long in the hospital, without receiving any relief.—Several of his joints are swelled and tense; and exquisitely sensible to every external impression.—His moanings generally accompany the changes of the moon.—He appeared worse about the beginning of this month, and on strict enquiry, I found the pains increased on the 29th, and on the morning of the 4th he had for the first time a quiet sleep.

Sometimes upon this man the diurnal changes are as evident and regular as the diurnal variations of the Barometer or magnetic needle.

JOHN CAMPBELL.

Calcutta General Hospital, 10th December, 1792.

Respecting the parallaxes—from Dr W. Hunter.

AT the full moon just passed, the parallax was very high. Have you heard of any remarkable effects from it? Although I had previously been in perfect good health, on the evening of the 15th after dinner, at 10 o'clock I happened to go for a few minutes out into the air; and on returning into the house, I had all the sensations of an approaching fit of intermittent. The pulse also considerably quickened. I went to bed soon after, and was well in the morning. But the next day at noon, the same symptoms returned: they continued about an hour, and left a kind of lassitude all the afternoon. This led me to consult the ephemeris, when I found the moon was full on the 15th, at 21 minutes past 2 P. M. and that she came to the meridian at 22 minutes past midnight; so that my first attack was about 6 hours after the full, and 2 hours before the moon came to the meridian. The H. P. was then $60'. 22''$. The highest in this lunation being $60'. 54''$. and the lowest $54'. 15''$.

At the time of the second attack, (which was 21 hours 39 minutes after full moon,) the moon was precisely on the meridian below the earth, and the H. P. 60. 1. I had a slight sensation of the same kind at 1 P. M. the following day, after

which the moon having got at a distance from the syzygy, and the H. P. also diminished, I felt nothing more of it.

WILLIAM HUNTER.

*Calcutta, 20th January, **

Respecting the parallaxes—From Dr Fullarton, 1793.

THOUGH the commencement of the last lunar period did not shew itself so powerfully in the hospital, as might have been expected, its progress has been as strongly marked as any I ever remember to have observed.—Its effects began to appear in the night of the 31st.—On the following day, I had 3 very severe and unexpected relapses in my ward. One of them was in a man, who had been discharged, cured in the morning, and was only waiting to be sent to the garrison in the cool of the evening. Another was in a patient labouring at the time under a very severe salivation, a state in which I hardly ever knew an attack of fever take place, certainly never one so violent.—During the succeeding days, all the fevers in the ward (almost without exception,) underwent a marked exacerbation, and the lingering invalids who had been accustomed to attacks in the springs for months back, had them more than usually severe.—Two of my patients were in a more alarming state than any I have had this season, and one of them I believe was only saved by practice more active than common; all the chronic complaints in the ward were exacerbated; the patients whose mouths had been slightly affected with mercury, were in several striking instances, (even where the mercury had been intermitted), seized with violent salivation, and their faces swelled up to their eyes;—and one miserable object, who has been in the hospital with chronic lues for a twelvemonth past, and has of late been subject to bleedings from the nose, chiefly at the lunar periods, was seized with so violent a fit of it 4 nights ago, that they were obliged

* The year is omitted here, and I can't venture to supply it from recollection,
F. B.

to raise me from bed, and I was forced to employ plugs of sponge dipped in a solution of alum, before I could stop the flow.—Nor in any of these cases, was there any general remission to be observed on the going off of the springs: on the contrary, they rather continued to get worse. It was not till yesterday morning, when the parallax came down to $59^{\circ} 38'$, that I could perceive any remarkable change. The remission became then at once universal; and this morning there is not a man in my ward (though it is pretty full) who is seriously ill,—not above 2 or 3 who have had any nocturnal paroxysm.—The observations of the other gentlemen coincide with mine, though I cannot acquaint you with the particulars.

I regret that the bustle I am in at present, deprives me of leisure to give you a more methodical and minute statement.—You already know I suppose that I have at length succeeded in obtaining a confirmation in my appointment, and I am just now busily preparing to change my residence.

The expectation of this removal taking place; the inactivity that the season naturally produces, and the small number of cases fit for observation, that (till of late) have been under my care, have hitherto discouraged me from resuming the more minute lunar journal, which I carried on in the spring. I hope I shall have perseverance enough to do something further, when I am fairly settled on the other side of the river.

I am,

Dear Sir,

JOHN FULLARTON.

Calcutta General Hospital, September 7th, 1803.

Respecting the parallaxes—From Dr John Campbell, 1793.

This full moon has had a more powerful effect upon the wards, than any change since the last time I made you a report upon that subject.

The patients are generally languid, and require a larger than usual quantity of wine, to support the natural functions, and assist the operation of medicines, several of those, already much reduced, have sunk gradually as if under the operation of a narcotic, and their bodies emit a more than usual degree of foetor before death.

The admissions from the 30th to the 6th in the sailors ward are more than double the number for the preceding week ; but the cases are not confined to fever ; there are many old fluxes, pains, and ulcers, of late considerably aggravated.

During several months attention, I have not been able to discover that the paroxysms of intermittent fevers, follow the cause of the tides as an effect, tho' they may have some connection with it.—The influence, however, of every change upon, I may say, all bad cases, has been real and indisputable by any person who will take the trouble to observe it.

The average admission into the General Hospital for *two weeks* preceding the 30th August was 7 and a fraction per day ; one week after do. do. do. 13 and do. do.

Extracted from the books.

JOHN CAMPBELL,

SENIOR ASSISTANT SURGEON.

Calcutta General Hospital, 9th September, 1803.

NO. 74.

THE following extract, from a short account of the late malignant fever in Philadelphia, by Mathew Carey, 23d November 1793, is inserted here, because it serves to illustrate a phaenomena relating to the autumnal equinoctial period, which I have often observed in *Bengal*.—" It may be necessary to relate here, that the disorder raged with increased violence as the season advances towards the mild autumnal months. In September the mortality was much greater than in August ;

and still greater to the 25th of October, than in September. Altho' all the hopes of the citizens rested on *cold* and *rain*, especially the latter, yet on the 26th of October, the day on which the violence of the disorder entirely abated, *there was hardly any rain and but a very moderate degree of cold.*

The following is a list of the BURIALS,

In August,	325
September,	1442
October,	1993
November,	118

☞ With respect to this report it is to be observed.

1st. That in October 1793, the full of the moon happened on the 19th. 2 h. 54m. evening.

2dly. That this sudden change seems to have been connected with the going off of the springs.

3dly. That, if the crisis of fevers depended on a determined number of days, required for the *concoction* of the morbid matter, as has been supposed, in different cases, some requiring 7 days, some 14 days, and some 21 days—this sudden cessation of the fever could not have taken place all at once, as in this case. Sol-lunar influence, only, can explain this, and is *alone* sufficient.

NO. 75.

Sol-lunar Influence universally observed at BOMBAY, and on all the western side of India.

“ THE influence of the moon on the human body has been observed in *this part of India* by every medical practitioner*. It is universally acknowledged by the Doctors of all colours, of all easts, and of all countries. The people are taught to believe it in their infancy; and as they grow up they acknow-

* It must be remembered that the gentleman who writes this letter, and who speaks for the whole medical establishment of the western parts of India, was then at the head of the medical corps; had resided there not less possibly than 25 years; and that his testimony in every respect is of the first class.

ledge it from experience. I suppose that in the northern latitudes this power of the moon is far less sensible than in India: and perhaps less so in Bengal, than in our neighbourhood. We here *universally* think that the state of weakly and diseased bodies is much influenced by the motions of the moon. Many people know the very day on which these intermittents will make their appearance; and every full and change increases the number of the patients of every practitioner. It is no argument against this influence, that diseases appear during every day of the month. The human body is subject to alterations from a thousand external circumstances, and from many affections of the mind. These lay the foundation of disease at every period; but they do not overthrow the evidence of lunar influence: altho' they are apt to mislead with regard to effects that depend on that alone. That the human body is affected in a remarkable manner, by the changes of the moon, I am perfectly convinced, although I cannot constantly pretend to see the operation of the general law; nor to account at all times for its perturbation: and agree in thinking that an attention to the power of the moon is highly necessary to the medical practitioner in India*."

HELENUS SCOTT.

Bombay, 6th May, 1801.

Conceiving it to be altogether superfluous to accumulate farther proofs of the influence of sol-lunar power in ASIA, I shall now endeavour to collect as many evidences of its action in the other quarters of the globe, as may be sufficient to shew that it is not a partial, but an universal law of nature.

Evidences of sol-lunar influence in AFRICA.

NO. I.

HAVING, in 1789, had an opportunity of observing in myself and several other cases, the evident effects of sol-lunar in-

* Dr Scott's observations certainly extend to all the Countries on the western side of India; and probably to Persia, and to the Coasts of Arabia, and the Shores of the Red Sea.

fluence at the Cape of Good Hope, and Dr Fontana having observed it in a wide field on the eastern coast of Africa *, I intended to have included in this number, along with these testimonies, the observations made by Dr M'Gregor on the expedition from India to Egypt; the importance of which, to the result of this investigation, I have already explained †.—Dr M'Gregor's Treatise, however, not being at present in my possession, I must be satisfied with allotting a place for it under this number, in some future edition of this volume.

Evidences of the power of sol-lunar influence in EUROPE.

NO. I.

“ Is an instance of pain returning regularly in the morning every day for the space of 26 years, and becoming also greatly increased at the periods of new and full moon; extracted from the history of a case read at a meeting of the Medical Society of LIVERPOOL, by John Rutter, M. D. physician at Liverpool: and published in the Edinburgh Medical and Surgical Journal for April 1808.”——“ The pain (which commenced in her 50th year,) was her daily companion for not less than 26 years. It began early in the morning, and frequently waked her out of her sleep, and continued to encrease until noon, when it appeared to have acquired its utmost violence; and towards evening gradually abated; but I never heard her say, that when awake, she was altogether free from it.—The pain was also greatly increased at the periods of the new and full moon. *She* first directed my attention to this circumstance, and I observed it for many years afterwards to recur with a degree of regularity, which leaves no room to doubt the fact; to whatever cause it is to be ascribed.—There is something singular in the regular exacerbations of the pain every day, as well as in those that were observed at the lunar periods. I know no explanation that can be given of this.”

* Vide NO, 54.

† Vide page 200.

THE STATEMENT OF FACTS.

I.

IN taking a review of the preceding collection of authorities, we discover,

FIRST, That almost the whole of them † concur in affirming the truth of the following observation, viz. “that the relapses and
“exacerbations of remitting and intermitting fevers, happened much more frequently at the springs, about the full and
“change of the moon, than in the neaps.”

This observation necessarily implies, that all those who made it, and communicated it in their letters, did absolutely observe the four following separate and distinct facts, without the previous observation of which they were not warranted, and therefore could not possibly have been induced to make this affirmation. The facts are these,

1st. That *many* relapses and exacerbations happened during the springs, at new moon.

2d. That *few* relapses and exacerbations happened during the neaps following new moon.

3d. That *many* relapses and exacerbations happened during the springs, at full moon.

† At least 64 numbers out of 70—by all excepting NO. 10, 30, 38, 55, 66, 69.

4th. That *few* relapses and exacerbations happened during the neaps following full moon.

II.

SECONDLY, we discover that the four following facts are also confirmed by a considerable number of the same authorities ‡, by my own experience for more than twenty years; and by the testimony of many with whom I have conversed on this subject; viz.

5th. That during the springs, *many* paroxysms come on about the middle of the day: viz. between half past eight A. M. and half past three P. M.

6th. That during the springs, *few* paroxysms come on in the evening between half past three P. M. and half past eight P. M.

7th. That during the springs, *many* paroxysms come on about the middle of the night: viz. between half past eight P. M. and half past three A. M.

8th. That during the springs, *few* paroxysms come on in the morning between half past three A. M. and half past eight A. M.

III.

THIRDLY, although the tendency of febrile paroxysms to postpone their attacks, has not hitherto attracted the notice of physicians, and is only supported by a very few observations in the preceding collection *; yet upon this foundation, with the strength which they derive from their close connection with other facts already established, I venture to add the fol-

‡ Not less than 17 numbers, viz. 1. 2. 3. 5. 7. 8. 15. 17. 19. 25. 28. 32. 33. 39. 49. 63.

* In NO. 1. 22. 25. 26 NO. 71.

lowing to that number, without any apprehension of being mistaken.

9th. That both during the springs and neaps, the paroxysms of fevers have a *tendency to postpone* the hour of attack at each succeeding return, in coincidence with the tides.

The induction required to establish this fact completely must consist of observations concurring to prove, that this tendency prevails on every individual day of the springs and neaps, or at least on a majority of them; and I hope they will be recorded with accuracy by future observers. The observations which I have made myself are pointed with regard to the four first days of the neaps. On other days they have only been occasional and uncertain; but still contributing, in my opinion, to shew the general tendency.

IV.

By establishing and discriminating these *nine* different facts, it will not escape the observation of those who are at all acquainted with the present state of medical science, that we have acquired the knowledge of no less than *nine different remarkable coincidences between certain phaenomena of fevers, and certain periods of time, hitherto overlooked, and unknown in medicine and natural philosophy.*

V.

The reality of the 1st, 2d, 3d, and 4th of these coincidences is confirmed by the testimony of no less than 64 gentlemen, of unquestionable veracity and honour, possessing frequent opportunities of observing them in themselves and others, in every quarter of India, for months and years together; and is not contradicted by a single authority.

VI.

The reality of the 5th, 6th, 7th, and 8th coincidence is confirmed by testimony which is also unquestionable. They have

been observed by many of the gentlemen who testify the existence of the other coincidences : those who have not observed them, acknowledge that these are circumstances to which they have not directed their attention, and may, therefore, have overlooked them ; and in the course of *ten* years, there has not appeared a single individual who declares, that he has looked for these coincidences *with care*, and has not been able to discover them.

For my own part, I say with confidence, that they are confirmed by the recollection of what I saw from 1769 to 1784 ; that from 1784 to the present time, during which interval I was led to attend more particularly to this question, I have met with nothing that I considered as any exception to these rules ; and it is of importance to add, that they are confirmed by the result of the observations and reading of Dr William Cullen, who, in forming his *Nosologia Methodica*, it is probable, took pains to consult, upon this very point, the works of every author of character that he could procure in Europe †.

VII.

With respect to the reality of the 9th coincidence, although I be not able at present to support it by any positive testimony, excepting that of the *numbers* § which I have already specified, and of my own observations, it must be considered, that it stands uncontradicted ; and further that it is rendered highly probable by the existence of all the preceding coincidences, the reality of which we have established on evidence that cannot well be suspected or shaken.

At the same time, if, notwithstanding all these circumstances, any doubts should still be entertained respecting the existence of this coincidence, or the propriety of reasoning n-

† Vid. First Lines. parag. LVI.

§ That is to say, the different letters, &c. in this collection.

pon it, in its present state, as an established fact, we shall cheerfully consent to set it aside: because it is not at all necessary to the inference which we are entitled to draw from those that remain.

INFERENCE.

VIII.

Between things that are *not connected* as cause and effect, a constant and regular coincidence in time and proportion, in so many different points, and in so many different examples, for months and years together, is a circumstance which does not occur in the history of nature; and therefore, agreeably to reason, is a coincidence that can never be expected to happen.

Upon these principles, every constant and regular coincidence in nature necessarily implies the existence of a proportionate corresponding cause. We are, therefore, led, naturally and irresistibly, to refer *those* that we have discovered and described to sol-lunar influence; which, from its action on the other parts of nature, appears to be perfectly adequate to this effect, and at the same time exhibits *intensions, remissions, and postponings*, in the exertion of its power, *coincident in time*, and *correspondent in degree*, with those which take place in the paroxysms of fevers.

Upon these premises we establish a proof of the operation of sol-lunar influence in fevers, which cannot be rejected, without violating the principles and rules from which we infer the existence of a cause in every other question and transaction in life.

IX.

If the truth of this inference should seem to require any farther confirmation, it will be found in observations made on the urine. In a *Treatise on the Action of Sol-Lunar Influence*,

published at Edinburgh in 1791, I have gone very fully into the history and theory of the urine in fevers (vid. from page 138 to 173;) and, by the assistance derived from the doctrine of sol-lunar influence, I have arranged the one, and explained the other, in a manner that appears to me to be perfectly agreeable to nature and truth. And, although I do not know that it has been as yet taken notice of either in Europe or in Asia, I cannot help believing, that, when it comes to be better understood, and compared with the ignorance and confusion which have hitherto involved this subject, *it will be considered as one of the greatest improvements that medicine has received in modern times.* In judging of fevers it is a criterion of great and extensive use, and, in physiology and pathology in general, points to objects that are important and new.

This investigation of the urine being too long to be inserted here, I must refer the reader to the Treatise I mention, and content myself with assuring him, “*that there is to be observed by any person who will take that trouble, in the appearance and consistence of the urine in fevers, a constant and regular fluctuation; that is to say, regular diurnal and septenary changes in its character, coincident and correspondent with the periodical intensions and remissions of sol-lunar influence.*” And as the doctrine of sol-lunar influence explains the periodical changes of the urine in a consistent and satisfactory manner, so do the periodical changes of the urine exhibit, in support of sol-lunar influence, a chain of coincidence so clear and connected, that it might be assumed upon this foundation alone, without any other collateral proof from the other symptoms, or the other parts of nature.

X.

When the present theory of sol-lunar influence was first published in Bengal, about ten years ago, it was assumed by those who opposed it, that the mercury in the barometer did not, by any regular rising or falling in coincidence with particular periods of time, indicate any periodical changes in the state of

the atmosphere, corresponding with the revolutions of the sun and moon. From this it was directly inferred that, "as the revolutions of the sun and moon produce no changes in the weight and pressure of the atmosphere which surrounds the human body, they cannot possibly have any influence on the state of the human body itself; and consequently that the phaenomena which support this theory are merely imaginary, or at least accidental, and totally independent of any regular or permanent cause."

Of this proposition the premises happen to be false. From the observations of Mr Toaldo*, we have now the satisfaction to know, that certain periodical changes in the atmosphere and barometer do actually take place, which are regularly connected with the periods of the full and change of the moon,

* *Vid.* the abridged Exposition of the System of Mr Toaldo, upon the probability of the change of weather by the lunar points, translated from the *Journal des Sciences Utiles*, and published in the *Calcutta Magazine* for July and August, 1793.

Mr Toaldo, after collecting observations for 50 years, adjusted the relation which subsisted during this interval between the changes of the weather and positions of the moon, and that examination informed him, that fine weather coincided most frequently with particular lunar points, and that others were accompanied with tempests; and he takes notice of ten positions of the moon capable of producing a sensible effect upon the atmosphere.

The movements of the barometer are so analogous to the changes of the weather, that it was likely the moon had an influence on the heights of the mercury. To ascertain that circumstance, Mr Toaldo collected a journal of the observations of the barometer made during several years, the relations of which, with the occurrence of the lunar points, he carefully examined; the result is,

1st. That the barometer is a sixth of a line more elevated at the times of the apogees, than at those of the perigees.

2dly. That it is six tenths of a line higher at the times of the quadratures, than at those of syzygies; and,

3dly. That it is by a quarter of a line higher at the meridional, than at the septentrional lunistices.

and her different quarters. And the observations of Mr Trail* and Mr Farquhar† at Calcutta, with those of Sir William Beeston§ in Jamaica, and of M. De Luc || in Europe, place it beyond doubt, that certain periodical changes in the barometer happen regularly at particular stated times of the day, which are of course connected with certain relative positions of the sun and moon; and consequently that the state of the atmosphere may concur at certain periods in producing, or at least modifying, the periodical revolutions of febrile paroxysms.

But supposing the premises of the above proposition to be true, the conclusion which was drawn from them does not follow. It is easy to conceive, and, from the present aspect of chemical philosophy, not at all improbable, that a quality capable of producing the paroxysms of fevers may exist in the atmosphere, and undergo periodical intensions and remissions in its power, in coincidence with the revolutions of the sun and moon, without any obvious corresponding alteration in the weight or pressure of the atmosphere itself.

CONCLUSION.

XI.

FROM the sum of these premises we conclude, *that the dominion of sol-lunar influence over fevers is now demonstrated, and no longer to be considered as a matter of doubt; but that future inquirers should proceed directly to investigate the particular laws and restrictions under which it acts, for the purpose of improving our knowledge and skill in useful arts.* And thus I have performed what I proposed to accomplish in my FIRST VOLUME.

* Vid. the ingenious and accurate diary of Henry Trail, Esq. in the second volume of the Asiatic Researches.

† Vid. NO. 49. in this Treatise.

§ Vid. Encyclopaedia Britannica, Vol. II. page 1021.

|| Vid. Encyclopaedia Britannica, Vol. II. page 1029.

N. B. I have at this time materials prepared and arranged for my SECOND VOLUME; but being desirous to possess all the information I can procure, I have for this purpose forwarded copies of the following letter to the different stations of this establishment, and other parts of India; and shall therefore defer the publication of it for at least another year.

DEAR SIR,

FROM the observations contained in the many obliging replies which I have already received to my letter of the 22d of June, it appears to me that the existence of sol-lunar influence is supported by a body of evidence that cannot be shaken.

Considering it, therefore, as an established principle, I find myself led forward to investigate the particular laws and restrictions under which it acts; and being confident that the prosecution of this inquiry will reward my labour, by throwing new lights upon many branches of useful knowledge, and above all on medicine, I am once more induced to solicit your assistance in bringing it to a conclusion.

After much reflection on this subject, I find that all that is required for my present purpose, is to ascertain with tolerable precision the hour of the day or night at which each paroxysm comes on; and that our observations may be sufficiently conclusive, it is necessary that they should be carried on through all the different seasons of the year, in an extensive field, such as that which presents itself in this country. I shall do my best to avail myself of this opportunity, and if I can prevail on my friends to continue, for a short while longer, the indulgence they have already shown to me in my first attempts, I pledge myself to exhibit a history of phacnomena in fevers that shall be instructive and useful, and not uncreditable to them by whose aid I may be able to present it to the public.

Our business, being almost solely with the *dates* of paroxysms, the trouble of keeping a proper record, agreeably to the *form* which is annexed, will be very inconsiderable, compared with that which is required in detailing medical cases.

Column 1st is allotted to this part of our task—and although the *dates* of those regular successions or paroxysms, which constitute complete fevers, will be the most instructive and useful, it will be also of great importance to record all those which appear singly or detached. If it can be done conveniently, it will likewise be of consequence to ascertain the duration of every paroxysm; and it may be recorded on the same line with the date, in the manner it is done in the *form*.

In column 2d is to be recorded the *state* of the paroxysms, whenever it can be done with certainty. It will serve to give a general idea of those periodical septenary revolutions which have been so little attended to by modern physicians; and for this purpose it will be sufficient to express it in the general terms specified in the *form*.

In column 3d is to be recorded, in a short memorandum, the operation of the medicines, and any other circumstances that may affect the *date* or *state* of the paroxysms, as it is done in the *form*.

In short our principal object is to ascertain the dates of febrile paroxysms of every kind; so that observations, although confined to this circumstance alone, will be highly acceptable; I hope, however, that where there is an opportunity, the other two columns will not be neglected.

I make these requests with much reluctance and diffidence, yet when I consider their nature and tendency, with many hopes; and remain,

DEAR SIR,

With esteem,

Your very obedient servant,

FRANCIS BALFOUR.

Calcutta, November 6, 1793.

JOHN SMITH, 1794.

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FACTS.

<i>Moon.</i>	<i>Oct.</i>	<i>Date of Paroxysms.</i>	<i>State of Paroxysms.</i>	<i>Operations of medicines &c.</i>
	1			
	2	The 1st began with cold at 11 a. m. and continued 2 hours.	Moderate	Report
	3	The 2d began with heat at 11 p. m. and continued 1 hour.	Moderate	Report
	4	&c.	Moderate	Report
	5		Moderate	Report
	6		Became more violent	Report
	7		Continued more violent	Report
	8		Continued more violent	Report
	9		Became moderate	Report
	10		Continued moderate	Report
	11	No return, &c.		

P. S. Two or three quires of country paper, being sewed up at one end, like a music book, will give the whole breadth of the sheet of a page, which will be more conveniently divided into columns agreeably to the form, than when sewed in the middle, in the common way. When you cannot attend to the paroxysms yourself, the patient, or a careful servant, with your instructions, will be able to note and report the hour of attack and remission with sufficient accuracy.

Dr John Hunter's Observations on the Diseases of the Army in Jamaica, &c. &c. Chapter VI. contains the following remark on sores and ulcers, viz. "that their appearances are continually varying. *At times* they acquired the look of healthy sores, and sent forth strong and luxuriant granulations, and began to skin over. *But one night* often put an end to this flattering prospect, the granulation turned flaccid, and even mortified in part, the portion skinned over, ulcerated afresh, and the sore became larger than ever. *After a time* it again put on a healthy appearance, and *repeatedly ran through the same stages*: till at last the bones became carious, and, if amputation was not performed, or the patient carried off the island, he died."

In this short history Dr Hunter has distinctly described a periodical fluctuation in the state of the sores and ulcers of Jamaica, which corresponds surprisingly with what has been observed in this country; and what I think is constantly connected with a certain febrile disposition, liable to exacerbations and remissions corresponding with the tensions and remissions, which are observed in the action of sol-lunar influence on fevers in general, during the lunar periods and interlunar intervals.

Considering the great frequency of sores and ulcers amongst soldiers, this is an inquiry, no doubt, of very great importance, and you will oblige me greatly by sending the result of your

observations on this subject along with those on fevers; which, however, I shall not be anxious to receive for these twelve months to come: for, to render them complete and conclusive, they should no doubt be carried on through all the variation of season that occurs in the revolution of one year.

FRANCIS BALFOUR.

POSTSCRIPT.

1. Of all the phaenomena which occur in the contemplation of animal nature, we need not hesitate to affirm, that the PAROXYSMS OF FEVERS are the most remarkable, the most formidable, the most interesting to mankind. In the space of an hour the most healthy and vigorous men are shaken and deranged; in the course of a few days powerful fleets and armies are discomfited, and flourishing cities depopulated and laid waste; and ultimately, by far the greatest part of the human race is swept away by this terrible affection.

2. The cause, however, which produces these wonderful effects, and determines the paroxysms of fevers to appear in different cases, in various order and succession, constituting fevers of DIFFERENT TYPES; and that again which determines fevers of different types to come to an end after certain lapses of time, forming these into fevers of VARIOUS DURATIONS, are questions which have baffled the research of physicians; and which have therefore lain for centuries, almost without discussion.

3. In the SECOND VOLUME of this work I trust that I shall be able to demonstrate, that all the DIFFERENT TYPES of fevers, that appear in nature, are formed by febrile paroxysms continuing to return in succession for a certain number of

days, at an interval of 12, 24, 36, and 48 hours, or some other larger multiple of 12 hours, and almost invariably in coincidence with the tides; that the types of fevers are therefore formed by the action of sol-lunar influence producing paroxysms in coincidence with the tides, at the intervals we have described: and consequently that they differ from each other, only in so far as their paroxysms return in a succession, interrupted by intervals formed by different multiples of 12 hours.

To convey a general idea of this *discovery*, I have constructed Table I; observing that it applies to explain all the types that I have ever met with either in the course of my reading or observation; that it agrees perfectly with the descriptions of Dr Cleghorn, one of the most accurate writers in medicine; and is confirmed by the general conclusions which Dr Cullen has formed respecting the accession of paroxysms, from all the knowledge he had acquired of this subject in the course of his life.*

4. Fevers of all the different types that are produced in the manner described in the first table, are limited to forms of *VARIOUS DURATIONS*, by the remarkable remission which takes place in the power of sol-lunar influence on the commencement of the neaps, and which brings them, at these junctures, to a termination or *crisis* whenever the state of the body is sufficiently disposed to concur in that event.

The operation of this law is explained in Table II. which exhibiting at one view all the more common variations that

* “ And that these paroxysms are connected with that diurnal revolution (a revolution which he supposes established by a law of nature in the human constitution), appears farther from this, that though the intervals of paroxysms are different in different cases, yet the times of their accession are generally fixed to one time of the day; so that quotidians come on in the morning; tertians at noon; and quartans in the afternoon.” *Vide Cullen’s First Lines, LVI.*

are produced in duration of fevers of a PERFECT TYPE, conveys also a clear and precise idea of all the different variations that may likewise be produced by the same law, in the duration of TYPES that are IMPERFECT; and shewing at one glance the whole of the vast variety of DISTINCT FEBRILE FORMS that may actually occur, will enable us to acquire a more perfect knowledge of their nature, and of the means by which they are to be prevented and cured.

5. This is a general view of the doctrine which arises from the *discovery* of sol-lunar influence. But *how* the power of this influence may be increased and diminished by various positions of the planetary system ||; *how* its effects may be modified by acting on different febrile or paroxysmal states different from each other in their nature and tendency; and *how* those again may be diversified by all the various accidents to which the human frame is liable from other causes, are subjects of too great extent for our present design, and are therefore deferred to a future occasion. By this short anticipation of what I have undertaken to perform, I mean only to present a clear and distinct view of *the land which is promised*; so that, whatever may now become of me, those who see it may go on to possess it, and give it to their children as an *inheritance* for ever.

☞ Immediately after the publication of this Treatise in India, a copy of it being sent to England, it was re-printed and sold on my account by Mr Murray, Fleet Street, London, in the course of 1795: and was entered at Stationers'-Hall, as the FIRST VOLUME of the intended work described in the Preface.

|| Mr Farquhar's letter, NO. 49, contains very important hints upon this subject, of which I shall avail myself hereafter.

TREATISE V.

ON

THE BAROMETER.

This Treatise first appeared in the 4th Volume of the transactions of the Asiatic Society, published at Calcutta in 1795.

ON
THE BAROMETER.

I.

IN a Treatise published at this place a few weeks ago on solar influence in fevers, I have endeavoured to shew, “*that all fevers are liable to certain diurnal and septenary* revolutions; and that these revolutions are uniformly and constantly connected with fixed periods of time.*”

II.

Having established this proposition (1) it was natural to suppose, that the power or influence which is capable of producing these very remarkable and interesting revolutions on the human constitution, at certain intervals, did not exert itself without effecting, at the same time, some corresponding periodical change in the state of that element in which we constantly exist; and in which all the operations of life and nature are carried on.

Other necessary avocations having hitherto prevented me from being able to make those experiments myself that are required for deciding on this question, I applied to Mr FARQUHAR, who I understood had paid some attention to this subject, and was favoured with the following very obliging and instructive letter:

☞ Mr Farquhar's letter being already inserted as NO. 49 in the preceding Treatise p. 257, is now omitted.

* That is to say changes happening after an interval of seven or eight days.

III.

Although in this letter Mr Farquhar describes in the barometer only *three* different diurnal periods of rising and falling, I could not help suspecting that there must likewise be a *fourth* which had escaped his notice; and that I should be able to discover a periodical falling, also, in the state of the mercury, between eleven at night and six in the morning, analogous to that which he had observed between eleven at mid-day and six in the evening. Accordingly, by keeping myself awake, and continuing my observations during the night, I have now the satisfaction to be assured, that my anticipation of the revolution I expected to discover was perfectly just.

IV.

With a view of ascertaining the progress of these *four* different revolutions by personal observation, I imposed upon myself the task of observing and recording the changes of the barometer, as far as I was able, every half-hour, day and night, during the period of one complete lunation.

The result of this undertaking I have now the honour to lay before the society; and if in matter or form it contains any thing worthy of their attention, or of a place amongst their researches, it will afford me a degree of satisfaction that will more than reward me for my labour.

I. OF THE PERIODICAL DIURNAL CHANGES OF THE BAROMETER.

The detail of facts.

V.

The detail of facts is comprehended in the following record of observations made on the barometer, as regularly as I was able to perform it every half hour, both day and night, during the lunation which intervened between the 31st of March and

the 29th of *April* 1794. To these I have added the state of the *thermometer* and *wind*, with the *appearance* of the sky.

VI.

My observations of the *barometer* were taken with scrupulous exactness, and although the weighty hand of sleep has more than once deprived me of observations that I was just about to make and was anxious to record, I have never ventured to assume any probable state of the mercury as an actual observation.

VII.

With respect to the *thermometer*, although it was liable to some inaccuracy from my not being able to preserve the apartment in which it was hung uniformly open or shut, yet, as the variations from this cause were trifling, and never obscured the regular and progressive rise and fall which it observes at different periods of the day, I conceive that my record is sufficiently exact for enabling me to decide, with safety, that the daily fluctuations which appeared in the barometer, were *not* connected with the daily vicissitudes of heat and cold.

VIII.

Although the state of the *wind* was not measured by any instrument, but estimated only grossly by the effect which it appeared to produce on the trees and other objects around, still I conceive, that I may also venture to determine on this ground, that the diurnal fluctuation of the mercury was *not* connected with the state of the wind.

In the column appropriated for recording the state of the wind, *Number* 1, represents a breeze capable of carrying on a ship two or three miles in the hour; *Number* 2, a breeze capable of carrying on a ship four or five miles; and *Number* 3, a breeze capable of carrying on a ship six, seven, or eight miles.

IX.

Neither are the *appearances of the sky* defined with much precision or minuteness ; yet upon the description that I have given, I think I may pronounce with sufficient confidence, that they did *not* direct or regulate the periodical diurnal fluctuation of the barometer.

By conceiving the wind, which in the month of *April* is generally from some point in the south, carrying constantly along with it, in the different degrees of velocity I have described (VIII,) different proportions of light and heavy clouds, we may obtain a tolerably just idea of the appearance of the sky at *Calcutta* during that month.

To express these different states, we have employed in the record the terms *clear*, *cloudy*, and *overcast*. When few clouds only appear, or none, which is seldom the case at this season, the sky is said to be *clear* ; when the sun or stars shine thro' a number of clouds, the sky is said to be *cloudy* ; and when the sun or stars don't appear at all, the sky is said to be *overcast*.

N. B.—As the record which details these different observations is too voluminous for this publication, I have been obliged to content myself with giving the results contained in the following pages *.

The Statement.

X.

The sum of my observations respecting the four periodical diurnal revolutions of the barometer which I have described,

* When I was leaving Edinburgh in 1799, on my way to India, I did myself the honour to send the Original Manuscript of this Record, to Professor Playfair, of that university.

appears at one view in the preceding synoptical arrangement,* and when stated precisely in numbers amounts to this.

1st. That on every day of the thirty comprehended in the record, excepting one†, the barometer constantly *fell* between ten at night and six in the morning; and that progressively, and without any intermediate rising excepting in one instance‡.

2d. That on every day of the thirty comprehended in the record, without one exception, the barometer constantly *rose* between six and ten in the morning; and that progressively, and without any intermediate falling, excepting in two instances§ ||.

3d. That on every day of thirty comprehended in the record, without one exception, the barometer constantly *fell* between ten in the morning and six in the evening; and that progressively, and without any intermediate rising in any instance.

4th. That on every day of the thirty comprehended in the record, excepting two¶ **, the barometer constantly *rose* between six and ten in the evening; and that progressively and without any intermediate falling in any instance ††.

* The original Table to which I here refer, being omitted in the republication of this Treatise, I cannot now replace it; but it is to be found in the IVth Volume of the Asiatic Researches.

† Between the 20th and 21st—*Vid.* Synopsis.

‡ Between the 22d and 23d—ditto.

§ On the 11th, - - -ditto.

|| On the 23d, - - -ditto.

¶ On the 15th, - - -ditto.

** On the 20th, - - -ditto.

†† My observations agree also with those of Mr Farquhar and Dr Mosely in this, "that the mercury remains stationary, for some hours, at its lowest and highest degrees."

The inference.

XI.

From the preceding statement of the coincidences observed in these four portions of the day, it appears that we may reasonably infer the following propositions, limited to *Calcutta* in the month of *April* 1794.

1st. That, in the interval between ten at night and six in the morning, there existed a *prevailing tendency* in the mercury to *fall*.

2d. That, in the interval between six and ten in the morning, there existed a *prevailing tendency* in the mercury to *rise*.

3d. That, in the interval between ten in the morning and six in the evening, there existed a *prevailing tendency* in the mercury to *fall*.

4th. That, in the interval between six and ten in the evening, there existed a *prevailing tendency* in the mercury to *rise*.

These different prevailing tendencies to rise and fall periodically, at certain times of the day and night, necessarily imply a proportionate corresponding cause sufficient to produce them. But here we stop and venture to proceed no farther than to say, with Mr Farquhar, that they seem to be connected with the diurnal revolutions of the planet which we inhabit.

XII.

By an attentive examination of the synopsis it will appear, that the general characters of the tendencies, which prevail at the different periods we have described, are liable, within their respective limits, to several remarkable *variations*, viz.

1. With regard to the *time of beginning* to rise or fall.
2. With regard to the *time of ceasing* to rise or fall.

3. With regard to *the steps or degrees* by which the mercury rises or falls.
4. With regard to *the limits or extremes* to which it rises or falls.

Being under the necessity of acknowledging our ignorance of the cause which produces these *prevailing tendencies themselves*, we can of course have no adequate idea or conception in theory, of the different circumstances that are capable of producing the different *variations* which appear in their general character; and our observations being much too limited to establish concerning them any thing like practical rules, we must remain contented for the present with pointing them out as questions which want investigation; expressing however a strong suspicion, that they are not unconnected with the relative positions of the moon, and the other planets.

The application.

XIII.

At the time of digesting the ideas which I have delivered upon this subject, being possessed of no information but that which was communicated in Mr Farquhar's letter, and what I obtained afterwards from my own observations, I did not conceive that I was authorised to extend the propositions which I have advanced (XI.) respecting these tendencies beyond the limits of *Calcutta*. By a note, however, which is just now pointed out to me in Dr Moseley's very ingenious Treatise on Tropical Diseases *, I have the satisfaction to find, that the very same tendencies have been observed to prevail on the opposite side of the globe. We may therefore now venture to allow them a more extensive range; and it will, no doubt, be considered of some importance to establish, in certain latitudes, † the existence of a law in nature by which the mercu-

* Vide the Note A. at the end of the Treatise.

† As far as I can judge from the following extract from Father Cotte's Memoir on the prevailing winds, &c. &c. which I have just met with in the *Edinburgh Magazine*, for March 1792, there seems to be great reason to believe,

ry of the barometer, let the standing weight and pressure of the atmosphere be what it may, is liable to the effects of a constant and regular periodical diurnal fluctuation: for it will then follow, *that* the power of each succeeding hour to raise or sink it, is liable to differ from that which went before; *that* the height of the mercury, therefore, taken only at two or three stated hours of the day, cannot, with propriety, be assumed to represent, or form a just estimate of the whole twenty-four; *that* calculations proceeding hitherto on such partial grounds, must necessarily include error and require adjustment, and *that* in future, wherever this law extends, no correct philosophical investigation, connected with the nature of the atmosphere, can be carried on, without giving it a place ‡; and no *just prognostic* formed of the weather without distinguishing those regular and constant changes from such as are only occasional and temporary.

With respect to medicine, this law is a principle entirely new; and it has now become a matter of real consequence to ascertain, in what respects it co-operates with the power of the sun and moon in producing and regulating the paroxysms of fevers. From the striking coincidence of *these tendencies*, with the periods at which the paroxysms of fevers generally

that similar fluctuations take place in the mercury in the different latitudes of *Europe*; and that they are not entirely confined to the regions under the equator.

“ The mercury is generally a little lower about two o’clock in the afternoon, than at any other time of the day; and it is highest towards eight o’clock at night. I would compare this fact without pretending to draw any consequences from it, with the phaenomenon of the magnetic needle, the greatest variation of which from north towards west, takes place about two or three in the afternoon, and the least about eight o’clock in the morning ”—*Vid. the Edinburgh Magazine, for March 1792, page 211.—Par. 6.*

‡ A mean extracted from means obtained from the extremes of these different diurnal fluctuations, will give the mean weight of the atmosphere much more correctly than the common process.

attack and remit, and from their superior prevalence in tropical climates where the paroxysms of fevers are also most prevalent, "*it seems to be highly probable, that they may have a considerable share in constituting that power which shews itself in so remarkable a manner in this country, and which we have denominated sol-lunar influence.*"

II, OF THE PERIODICAL SEPTENARY CHANGES OF THE BAROMETER.

XIV.

Respecting periodical septenary changes in the state of the barometer, the only information I have been able to obtain, is extracted from an abridged exposition of the system of Mr Toaldo, upon the probability of the change of weather by the lunar points taken from the *Journal des Sciences Utiles*, and published in the *Calcutta Magazine*, for *July* and *August* 1793. Mr Toaldo, it appears, in order to ascertain whether the moon had any influence on the mercury, collected a journal of the barometer kept for seven years, from which he discovered that the barometer was six-tenths of a line higher, at the times of the quadratures than at the syzygies.

If this journal was kept correctly on a proper plan, periodical septenary changes in the barometer, connected with the revolutions of the moon, are established of course. *But* if it was kept in the ordinary way of assuming two or three observations taken in the course of the day, to serve as a standard or rule for estimating the state of the whole twenty four, it is evidently *liable to errors*, which render the calculation precarious and inconclusive for the reasons already explained; which, however, had not occurred to me at the time of writing my last Treatise on Sol-Lunar Influence.

That the barometer will be differently affected at the springs and neaps, is an anticipation which has in its favour the strongest probability that analogy can afford. Yet upon a review of

the observations collected during the springs and neaps of the lunation which I have observed, I cannot say, that when arranged as they stand in the synopsis, in coincidence with their respective periods, they exhibit a difference of character to establish this conclusion. We therefore leave it to the decision of a far more extensive experience, conducting its observations on a plan similar to that which we have exemplified in this Treatise.

A.

The note referred to in Dr Moseley's Treatise is this :—"It has been observed in these and more equatorial regions, that though the barometer is useless in indicating the variations of the weather, it exhibits a phenomenon not correctly ascertained in temperate climates; which is, that the mercury has two diurnal motions of ascent and descent, of nearly a line corresponding with the course of the sun; ascending as the sun approaches the zenith and nadir, and descending as the sun deviates from these points. It remains stationary at its lowest and highest degrees for some hours."

In looking over Dr Moseley's Treatise on this occasion, I am sorry to discover, that trusting too much to memory, in referring to his work in my last publication, I have given a very imperfect account of what he has communicated on the subject of sol-lunar influence. But when he considers, that by my inaccuracy I have deprived myself of the weight of his authority in supporting a proposition I was anxious to establish, he will be inclined to ascribe it to the cause I have stated. Dr Moseley's observations are contained in the *conclusion* to his Treatise, between the page 550 and 556. They confirm the power of sol-lunar influence in Europe, in a very unequivocal manner, and merit the attention of those who wish for information on this subject. They relate to a very interesting case of Hæmorrhage.

TREATISE VI.

OF

SOL-LUNAR INFLUENCE

IN

THE FEVERS OF INDIA.

This Treatise was read before the Asiatic Society, at their meeting on the 7th of July, 1802, and was printed as the first paper of the VIIIth Volume of their transactions, early in 1803. —The object was to convey, in a familiar form, a connected idea of the whole theory ; with the scheme of an Ephemeris.

OBSERVATIONS, &c.

WHILST the interesting and successful researches of the Asiatic Society are exciting the curiosity and expectation of the learned in every quarter of the world, it is natural for those who are prosecuting discoveries in medicine and meteorology to look towards *India*, for some information respecting the nature and peculiarities of the climate in which we live. Possessing, as we do, the peculiar advantages of a tropical situation, with a more extensive field, and greater convenience for making observations than any *European* nation ever enjoyed before, it is an expectation which they have reason to entertain, and which on that account, and many other considerations, we ought, if possible, to gratify.

One of the most striking and interesting peculiarities of this climate is the wonderful connection that subsists between the paroxysms of fevers, and certain relative positions of the sun and moon; and as it is a peculiarity that leads to new ideas respecting the theory and treatment of the whole class of febrile diseases, and suggests *desiderata* for meteorological research; and therefore presents to the physician and philosopher, one of the most important phaenomena in nature, I have chosen it for the subject of this paper.

I. *Of the number and importance of the diseases that belong to the class of fevers.*

As the terms *fevers*, *febrile diseases*, or *class of fevers*, cannot convey to those who have not professionally or regularly applied themselves to the study of medicine, any just or ade-

quate idea of the great extent and magnitude of this subject, I have thought it expedient to take this occasion to observe, for their information, that the *class of fevers*, or *febrile diseases*, comprehends, not only the disorders that always receive the appellation of fevers, but a very great number of *others* that are never distinguished by this name: although the *fever* which accompanies them, constitutes the very *essence* of the disease.

Diseases of this description, of which many are far more destructive to the human race than those expressly called fevers, are most of them included in the following catalogue.

The plague, putrid sore-throats, epidemic catarrhs, dysenteries, pleurisies, peripneumonies, cholics, cholera morbus, acute liver, the small-pox, measles, erysipelas, elephantiasis, rheumatism, gout, tooth-achs, ophthalmias, megrims, obstructions of the liver and spleen, diarrhoeas, consumptions, spitting of blood, and haemorrhoids: many species of hypochondriasis, insanity, epilepsy, tetanus and asthma; the state of teething in children, all local inflammations, external and internal, accompanied with fever of any kind, and all sores and ulcers, especially of the legs in warm climates. In short all diseases attended with periodical exacerbations of fever, however obscure, &c. &c.

With whatever success, therefore, I may have acquitted myself in my *researches respecting the class of fevers*, it will appear from this explanation, that the object, at least, cannot, with truth, be represented as unimportant and useless. It cannot be unimportant and useless to investigate the nature of a class of diseases, by which the whole of the human race is sorely afflicted, and ultimately three-fourths of mankind are carried to the grave.

II. *Of the effects of Sol-Lunar Influence in fevers, denominated continued, remitting and intermitting.*

A collection of all the observations I have made on this subject, would be much too voluminous for a place amongst the researches of the Society. For my present object, it will be sufficient to state, as briefly as possible, the general conclusions that I have been led to draw from a view of the whole; and they are those that follow.

1st. OF THE PAROXYSMS OF FEVERS.

In *Bengal* there is no room to doubt that the human frame is affected by the influence connected with the relative situations of the sun and moon. In certain states of health and vigour, this influence has not power to shew itself by any obvious effects; and in such cases its existence is often not acknowledged. But in certain states of debility and disease, it is able to manifest itself by exciting *febrile paroxysms*: and the propensity or aptitude of the constitution, to be affected with febrile paroxysms in such cases, may be denominated the *paroxysmal disposition*.

From the great variety that appears in the violence and repetition of paroxysms in different cases at the same juncture of time, when the exciting power must act equally on all, it must be inferred, that the paroxysmal disposition exists in different cases in various degrees of *propensity*.

It appears also, from the history of fevers, that there is a disposition in all of them, which gradually increases and advances to a state in which it becomes *ripe*, or prepared for that remarkable change which terminates in a solution of the fever; and is denominated a *crisis*. This tendency in fevers may be called the *critical disposition*: which distinguishes it-

self in different cases, and at different times by various degrees of *maturity*.

The constitutions that prevail in different kinds of fever, discover obvious peculiarities with respect to the progress and *maturation* of the critical disposition. But that which is most important, and most material for the object of the present explanation, is a peculiarity that shews itself in the critical disposition of the common *typhus*. In cases of this fever, which is that which prevails in crowded cities, and in jails, ships, and hospitals, in all countries at all seasons, and is by far the most common, it is well established by experience, that the fever being once commenced, the paroxysms are very rarely disposed to cease in less than four days, and seldom so soon; and are not in general inclined to continue more than twenty-one.

The laws that regulate the progress and *maturation* of the *critical disposition*, in that constitution which prevails in *remitting and intermitting fevers*, which are generally attended with large secretions of bile, and are the endemic fever of warm climates, have not been as yet ascertained by any precise rules respecting their duration. But it appears to me that, whenever there are free discharges of bile, there is always a greater tendency towards a crisis or solution of the fever, than when there appears little or none, which is generally the case during the height of the *typhus*; and until some approach towards a crisis either perfect or imperfect has taken place: and the peculiar paroxysmal, as well as the critical disposition in all remitting and intermitting fevers, giving occasion to forms of different type and duration, may perhaps be connected with different states of the liver, peculiar to each.

2d. OF THE TYPES OF FEVERS.

OF PERFECT TYPES.

Febrile paroxysms universally discover a tendency to ap-

pear and disappear in coincidence with those positions of the sun and moon, that regulate the rising and falling of the tides.

The diurnal and nocturnal increase of sol-lunar power acting on constitutions, in which the propensity of the paroxysmal disposition is complete and perfect, produces paroxysms every twelve hours in coincidence with the periods of the tides*; and constitutes *types*, which on account of this regular coincidence, I have denominated *perfect*.

OF IMPERFECT TYPES.

The diurnal and nocturnal increase of sol-lunar power acting on constitutions in which the propensity to paroxysm is incomplete or imperfect, has power only to produce paroxysms in coincidence with every second, third, or fourth period of the tides, or others more remote; constituting *types*, which on account of this irregular coincidence, I have called *imperfect*.

By the discovery of this simple and universal principle, we are able to unfold the whole mystery of types; and to explain all the diversities that have appeared under the distinctions of *continued*, *remitting*, and *intermitting* fevers. Fevers hitherto denominated continued fevers, and supposed from the obscurity of their remissions to have none, are all of them to be considered as nothing else than fevers of a perfect type, in which two daily remissions may always be discovered, by attending to the remissions of sol-lunar influence, especially those of the morning; and fevers having paroxysms every twelve hours with obvious remissions, whether denominated continued or remitting fevers, are also evidently fevers of a perfect type.

* I express myself in this manner for the sake of brevity, meaning that the paroxysms occur in coincidence with the positions of the sun and moon that occasion the tides. The tides, it is well known, do not coincide with those exactly; but follow them a considerable time after.

Fevers in which the paroxysms do not succeed each other in twelve hours (and which have been hitherto denominated intermitting fevers when the remissions were complete, and remitting fevers when they were not) all belong to the class of imperfect types.

For the purpose of illustrating these explanations respecting types, I have constructed Table I.

2d. OF THE DURATIONS AND CRISES OF FEVERS.

OF THE DURATIONS AND CRISES OF FEVERS OF A PERFECT TYPE.

Febrile paroxysms shew themselves more frequently during the period of the spring tides than at any other time, and as these advance become more violent and obstinate; and on the other hand, tend no less invariably to subside and terminate during the neaps.

By the concurrence of the remarkable and sudden *remission* in the power of sol-lunar influence at the commencement of the neaps, with *critical dispositions in a state of perfect maturity*, all the different perfect types, produced in the manner I have explained, are brought to a final termination or *perfect crisis*; and are thus limited to fevers of *different durations*.

The operation of this law is explained in Table II. which exhibiting examples of the different durations of perfect types, with the manner in which they are formed, unfolds at one glance, the dark and once impenetrable secret of *crisis*; and accounts for all the diversities that may appear in their duration at different times.

An application of these principles enables us to explain in a similar and consistent manner, the formation of *crises* that

have been called *imperfect*. It is obvious, that whenever the remission in the power of sol-lunar influence at the commencement of the neaps acting equally on all, produces in some cases *perfect crises*, and in other *crises* that are *imperfect*, that the latter must be referred to the immature and unprepared state of the critical disposition to concur completely in that event. And although perfect crises, owing to the cause which I now mention, do not always take place at such junctures, *yet no fever*, as far as my experience goes, ever passes the commencement of the neaps without some evident abatement or remission in the degree of its violence; or without exhibiting some evident approaches towards a solution or crisis; and they are *approaches* such as these, in which the critical disposition concurs only partially and incompletely with the remission of sol-lunar power, that constitute those changes in the state of fevers that have been hitherto denominated *imperfect crises*.

This explanation respecting the nature of imperfect crisis being premised, I have now to observe, that although Table II. exhibits only such forms of perfect types as terminate by a final perfect crisis on the commencement of the neaps, it will now be well understood, that all fevers do not terminate finally and completely at this juncture; but that, in many cases, the *crises* being *imperfect*, the paroxysms continue to return for some time in a more moderate degree, and generally postponing with the periods of the tides, subside, and at last disappear gradually and imperceptibly. The *imperfect crises of perfect types*, such as these which I have just described, being less distinctly marked in their form, I have not attempted to represent them by any diagram.

OF THE DURATIONS AND CRISES OF FEVERS OF AN IMPERFECT TYPE.

For the same reason I have not attempted to reduce, to a synopsis or table, the *durations* and *crises* of imperfect types;

and because I am perfectly satisfied, that the same principles are equally applicable to explain the whole.

III. *The preceding theory extended to the whole class of febrile diseases.*

In prosecuting this analysis, we have obtained the knowledge of three very important principles in the pathology of fevers.

1st. That the paroxysms of fevers are produced by the action of *sol-lunar influence*.

2dly. That there is, however, a certain state of the human constitution, denominated the *paroxysmal disposition*, required to concur with the exacerbations of sol-lunar power in exciting and reiterating paroxysms, in such a manner as to form fevers.

3dly. That in the course of the disease there takes place in the constitution a certain state, denominated the *critical disposition*, which tending gradually to *maturity*, at length concurs with certain remissions of sol-lunar power in producing a crisis; by which salutary change, the tendency to paroxysm is diminished or removed, so as to bring fevers to an end, after certain intervals of time.

In my explanation of this theory, I have hitherto confined myself as much as possible to examples of the typhus, and of the endemic remitting and intermitting bilious fevers of this country; particularly those without local affection; and such therefore as are strictly denominated fevers. I now mean to extend it to every disease that is distinguished by febrile paroxysms, returning in coincidence with the periods of increased sol-lunar power, whether with or without local affection; and as there is no disease of the numerous list detailed at the

beginning of this paper, excepting the plague*, catarrhal fevers, and one or two more, in which I have not myself distinctly observed the coincidence of concomitant fever with the exacerbations of sol-lunar influence; the whole of that catalogue, and many others, though not generally distinguished by the appellation of fevers, are to be considered as nothing more than so many different modifications of fever; in which the peculiar constitution of each is variously affected by the action of sol-lunar power; and in such a manner as to produce the great variety of febrile forms that daily appear.

The exacerbation and remission of febrile paroxysm in coincidence with the rising and falling of sol-lunar power, constitutes the general and distinguishing character of fever or febrile disease; and although the lowest degree of this power, acting on paroxysmal dispositions in a high state of propensity, may happen to produce febrile paroxysms at an unusual period, such instances, though apparently exceptions, are no argument against the truth or principles of the general law: but are consistent with it in every respect.

Combining therefore the operations of the principles we have obtained from this analysis, we are enabled to construct a *theorem*, which serves to explain in a new, but satisfactory manner, the whole *class* of febrile diseases.

THEOREM.

“The fluctuating force of sol-lunar influence coinciding and co-operating in all its various stages and degrees, with the

* In several of the cases of the plague, recorded by Dr Patrick Russel, the febrile paroxysms returned obviously every twelve hours, in coincidence with the periods of the tides; and his predecessor and relation, the author of the *Natural History of Aleppo*, says positively, “that the generality of fevers there, and indeed almost all acute diseases, are subject to exacerbations once or twice in twenty-four hours.” *Vide Doctor Millar’s Observations on the prevailing Diseases of Great Britain*, page 203.

various modifications of the paroxysmal disposition, excites febrile paroxysms to attack on all the days of the neaps and springs, and supports and reiterates them, according to various types, until the commencement of different neaps; at which junctures the maturity of the critical disposition happening to concur with the periodical decline of sol-lunar influence, these paroxysms then subside and come to a termination or crisis: and thus form different successions of paroxysms constituting fevers of various length or duration."

It has been observed, respecting the various forms of durations, that some are apt to occur more frequently than others. To search for a solution of this question amidst the chaos of the incorrect and mutilated history that has been accumulated on the subject of fevers, would be unsatisfactory and useless. It will be far more profitable to observe their course with attention in future, when the laws that directs it are explained and understood, and I have no doubt that any physician who will carefully attend to the diurnal and nocturnal returns of the tides, and will constantly hold before him the prevailing tendency of fevers to appear at the commencement, and during the period of the springs; and on the other hand their prevailing tendency to subside and terminate at the commencement and during the period of the neaps; together with the observations that have been made respecting the propensity of the paroxysmal, and the maturity of the critical disposition, will soon obtain more information respecting the phenomena of fevers; and be able to form more just and certain judgments and prognostics respecting every event, than if he were to study the history of medicine as it is now written, for a thousand years. In short, there is no revolution or change in the course of fevers, that may not be explained by these general principles, in a manner that is consistent with the laws of the human constitution, and those of the great system of revolving bodies, which unite together in producing them.

Before I conclude this article, I must also recommend to every practitioner who wishes to emancipate himself from the beaten track, to attend carefully to the appearance of the urine; for I can assure them, from the experience of many years attentive observation, that there is to be observed, in the fevers of India, a constant and regular fluctuation in the colour and consistence of the urine in fevers. That is to say, regular diurnal and septenary changes in its character, coincident and correspondent with the exacerbations and remissions of sol-lunar influence.

The periodical fluctuation in the state and appearance of eruptions, sores, and ulcers in this country, being always connected with the periodical changes of a concomitant fever, an attention to these will be no less instructive than to those of the urine; and if the periodical changes of each were regularly and accurately delineated and expressed in colours with a pencil, by a judicious and careful observer, they would form a record in medicine and surgery of a new kind; which I have no doubt, would place the whole of this doctrine upon the basis of ocular demonstration; and afford to the most incredulous and inattentive perfect conviction of its truth.

IV. *Deviations from the prevailing tendencies of fevers during the periods of the springs and neaps.*

Although the general theorem, which I have advanced in the preceding pages, describe the prevailing tendencies of fevers during the springs and neaps, it is necessary to observe, that those tendencies are liable to frequent and remarkable deviations from the various stations that the moon may happen to occupy in her own orbit; by which her distance from the earth may be considerably increased or diminished: and consequently her power.

From observations lately made at the General Hospital at *Calcutta*, by Mr James Howison, Doctor John Campbell, and Doctor John Fullarton, it appeared, that the moon during the period of her greatest horizontal parallaxes had sufficient power to suspend, in a very conspicuous manner, the common tendency of the neaps to produce a remission of fever. And when the greatest horizontal parallaxes happen to coincide with the power of sol-lunar influence during the springs, we may reasonably infer, that the power of exciting and supporting paroxysms, must then be considerably raised above its usual force.

Besides the deviations that may arise from this cause, it is also reasonable to suppose, that the state of febrile paroxysms must be occasionally affected by every other change or perturbation of the moon's influence : but these are less remarkable, and have not been as yet ascertained by accurate observation.

V. *Of the state of fevers in India, during the equinoctial periods.*

I am now come to take notice of the remarkable appearances observed in fevers about the vernal and autumnal equinoxes. On this subject I have received from others very little information ; but I have not been inattentive myself to those periods ; and can pronounce with confidence, although my observations have not been recorded with regularity, that fevers are apt to occur more frequently, and with greater violence about both of those periods, than during the intervals either of summer or winter.

From these observations I was induced many years ago to advance, that the power of sol-lunar influence was considerably greater during the equinoctial periods than during the intervals either before or after them. It has therefore lately afforded me considerable satisfaction to discover, in De La

Lande's Astronomy, that De La Place has determined, from a very large collection of observation made by De La Lande himself, that the tides at *Brest*, about the time of the equinoxes, rise at a medium two feet higher than at the time of the solstices*. This discovery is agreeable to the general law of attraction; and it is not to be supposed, that the influence of the sun and moon, under the tropics, acts with a force inferior to that which produces this difference in the height of the tides on the northern shores of *Europe*.

How far sol-lunar influence affects the fevers of the higher latitudes of the globe, is a question that does not come within the scope of this inquiry. The annexed table†, however, extracted from Dr Currie, of *Liverpool's* medical reports on the effects of the water, &c. page 230, points so strongly to this subject; and is so immediately connected with the present article, that I could not resist the temptation of giving it a place; conceiving that it may become a stronger inducement to observation than any admonition or exhortation that I could offer.

Dr Currie's table was formed by him to shew the number of typhus fevers admitted into the *Liverpool* dispensary, in the course of seventeen years: and the admissions in that space of time amounted to no less than 48,367.

The great majority of patients admitted in the months of the spring and autumn, which I have denominated the equinoctial periods, compared with those admitted in the months of summer and winter, which I have called the inter-equinoctial intervals, cannot fail to attract the notice of every observer.

Without attending to fractions, we obtain from the facts established in this record, the following statement of admissions:—

For the mean of the equinoctial period,.....12,980

* *Astronomie* par Jerome Le Francois La Lande, Edition Troisieme Revue et Augmentee, Tome III. page 525.

† Table III.

For the mean of the inter-equinoctial intervals,.....11,232

For the common mean of those periods and intervals, 12,091

For the *rise* of the equinoctial mean, a-

bove the common mean,889, say $850 = \frac{1}{14}$

For the *fall* of the inter-equinoctial mean,

below the common mean,.....859, say $850 = \frac{1}{14}$

Those facts expressed in other terms amount to these:—

1st. That whilst the temperature of the season in the spring was passing from cold to hot, the number of typhus fevers *rose* about 1-14th above the common standard.

2dly. That whilst the temperature of the season in the autumn was passing from hot to cold, the number of typhus fevers *rose* in like manner about 1-14th above the common standard.

3dly. That during the months of summer, when the heat of the season is greatest, the number of typhus fevers *fell* beneath the common standard about 1-14th;—and,

4thly. That during the months of winter, when the heat of the season is least, the number of typhus fevers *fell* in like manner below the common standard in the same proportion, about 1-14th.

That the number of fevers should increase equally during the transition from cold to hot, as from hot to cold, and under the two opposite extremes of permanent heat and permanent cold, should equally diminish, are facts that are no doubt curious. At present, however, I mean only to suggest, that, if the theory of sol-lunar influence should ever be admitted in *Europe*, these phaenomena, apparently so very repugnant, may all be reconciled and referred to one common cause, without involving the smallest inconsistency or contradiction.

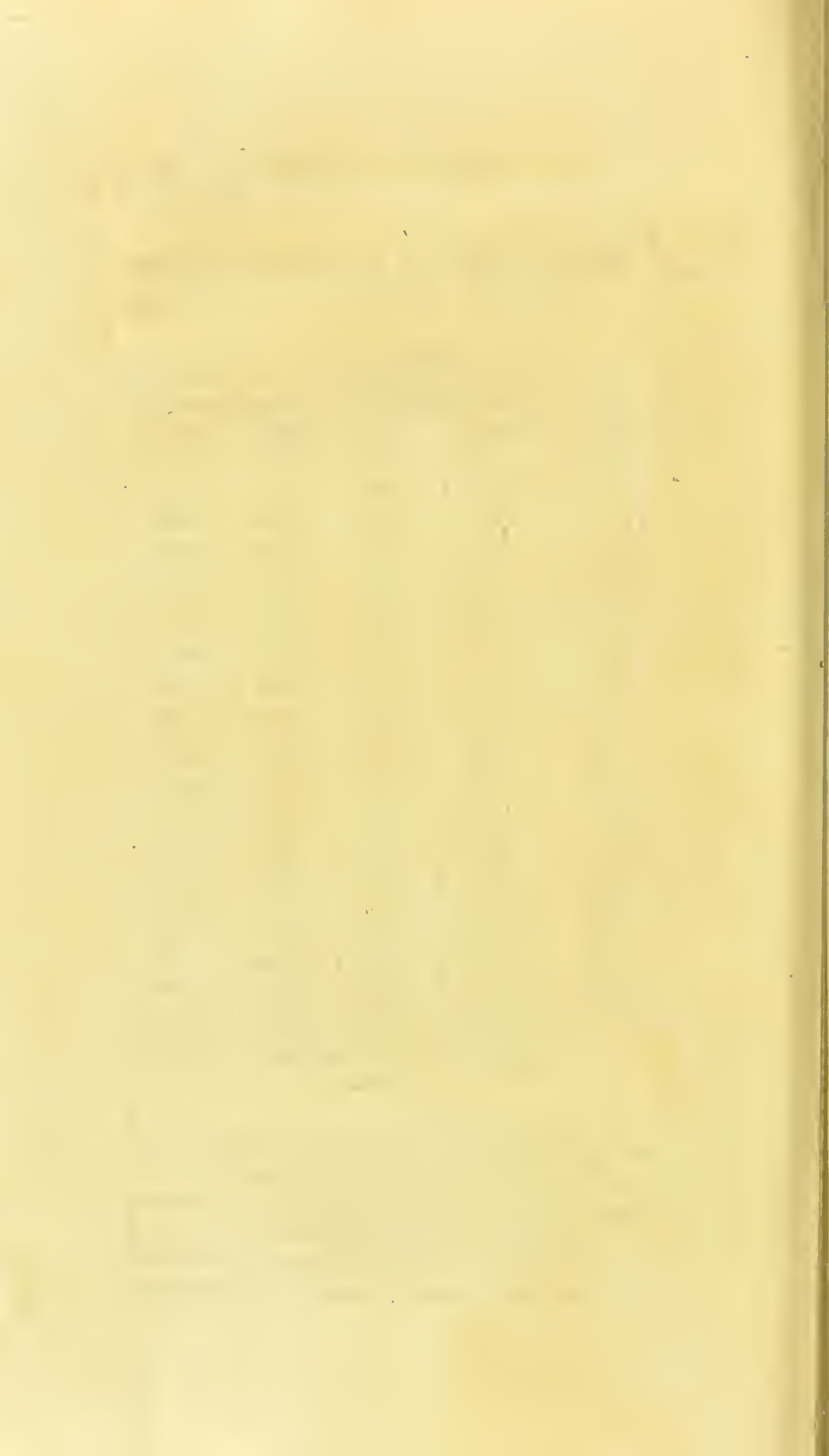
PLATE II.

To face Page 342.

Demonstrates the PERIODICAL INCREASE and DECREASE of FEVERS, in coincidence with the Equinoctial Periods and Inter-equinoctial Intervals.

Year.	The Vernal Equinoctial Period.			The Summer Inter-equinoctial Interval.			The Autumnal Equinoctial Period.			The Winter Inter-equinoctial Interval.			Total.
	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	January.	February.	
1780	179	173	168	183	191	150	129	186	150	133	130	146	1917
1781	180	200	187	154	157	127	167	234	208	223	268	265	2113
1782	231	292	148	159	120	140	143	182	150	158	210	158	2256
1783	184	207	122	212	136	227	255	316	257	273	170	194	2817
1784	245	247	232	225	270	230	256	247	369	297	285	268	2992
1785	296	294	219	187	173	10	186	250	244	182	191	166	2764
1786	216	244	202	155	159	188	169	211	167	197	256	209	2265
1787	301	234	313	356	255	192	218	234	283	326	236	174	3177
1788	213	235	253	245	271	311	258	341	315	295	319	176	3167
1789	338	323	391	205	184	162	212	214	204	208	176	248	2936
1790	337	294	281	247	343	270	310	340	355	269	253	247	3470
1791	277	230	233	240	266	248	300	344	335	371	359	361	3344
1792	269	278	261	237	236	223	211	330	212	174	174	209	3151
1793	221	259	237	334	199	197	338	305	224	227	157	230	2925
1794	383	280	337	305	291	245	303	290	258	326	152	265	3405
1795	546	204	234	230	248	159	196	239	317	180	197	161	2970
1796	266	242	288	176	203	182	254	329	153	247	150	125	2698
	4682	4236	4206	3450	3852	3431	4025	4592	4201	4017	3683	3602	48307
	13124			11133			12818			11332			

☞ THIS Plate is Dr CURRIE's II^d Table, divided agreeably to the doctrine of sol-lunar influence. In order to accommodate it more perfectly to this form, the columns of January and February are removed from the left to the right hand side, bringing all the three months of the Winter Interval together; and, to preserve the order in which the admissions followed each other, those two columns are raised one line higher; that is, the January and February admissions of 1781 are brought up upon the same line with those of December 1780, and follow after them as they really occurred; and so on with all the rest; and the January and February admissions of 1780, which stood at the top of these columns, are now placed at the bottom, to preserve the numbers of these columns entire.



VI. *Testimonies respecting the effects of sol-lunar influence in the fevers of India.*

As it is impossible on this occasion to detail at full length the various observations and arguments from which I have been led to adopt this theory, it is necessary to state, that it has not been taken up rashly; that it is now submitted to this society after the observation and reflection of thirty years; and that it is confirmed, in its most essential points, by the concurring observations of a large body of respectable gentlemen, whose names are contained in the following list*. And it is flattering to me to add, that Lord Teignmouth, who was then Governor General, conceiving that the correspondence of those gentlemen on this subject promised to be publicly useful, ordered my Treatise, containing their letters, to be printed and circulated at the expense of government.

Besides establishing unquestionable evidence of the general influence of this law in *Bengal*, these testimonies serve also to correct a very erroneous notion advanced respecting sol-lunar influence by Doctor Lind, by shewing that its effects in fevers are no less manifest at the distance of many hundred miles from the highest reach of the tides, than at *Calcutta*, and other parts of *Bengal*, to which the tides flow daily. The distances marked in the column, appropriated to that purpose, are very nearly the number of miles in a direct line between the places where the observations were made; and the utmost reach of the tides at the springs. Doctor Lind's theory made me anxious to ascertain these distances with precision; and the military surveyor general was so obliging as to direct it to be done at his office.

The information sent to me by those gentlemen, whose

* That list being already published in the 14th Treatise, page 196. is now omitted.

names are contained in the list to which I have referred, was all of it received in the space of a few months, in consequence of a circular letter, requesting observations on this subject, and on any side of the question, from those who might be inclined to give it. Several of those gentlemen I had never seen in my life; and with many I had the honour only of a slight acquaintance. Had I continued longer to collect testimonies, I am confident, that notwithstanding the diffidence and reluctance with which people commit themselves upon a topic of this kind, that I might have obtained in *direct* proof of sol-lunar influence, a much larger body of evidence, than is to be found in any single record in *direct* proof of the tides of the sea.

To accumulate testimonies of the remarkable effects of sol-lunar influence in *India* is now almost superfluous. In the western parts of *India* it is no less generally acknowledged than in *Bengal*: and I shall conclude this article, with an extract from a letter which I received some months ago, from a gentleman high in the medical line at *Bombay*; and no less so in the opinion of the public. His name, however, I forbear to publish, not having previously asked for his permission.

“ *Bombay, 6th May, 1801.*

“ The influence of the moon on the human body, has been
 “ observed in this part of *India* by every medical practitioner.
 “ It is universally acknowledged by the doctors of all colours,
 “ of all casts, and of all countries. The people are taught to
 “ believe it in their infancy; and as they grow up, they ac-
 “ knowledge it from experience. I suppose that in the north-
 “ ern latitudes this power of the moon is far less sensible than
 “ in *India*; and perhaps less so in *Bengal* than in our neigh-
 “ bourhood. We here universally think that the state of
 “ weakly and diseased bodies, is much influenced by the mo-
 “ tions of the moon. Many people know the very day on
 “ which their intermittents will make their appearance; and
 “ every full and change increases the number of the patients
 “ of every practitioner. It is no argument against this influ-

“ence, that diseases appear during every day of the month.
 “The human body is subject to alterations from a thousand
 “external circumstances, and from many affections of the
 “mind. These lay the foundation of disease at every period;
 “but they do not overthrow the evidence of lunar influence:
 “although they are apt to mislead with regard to effects that
 “depend on that alone. That the human body is affected in
 “a remarkable manner, by the changes of the moon, I am
 “perfectly convinced, although I cannot constantly pretend
 “to see the operation of the general law; nor to account at
 “all times for its perturbation; and agree in thinking, that
 “an attention to the power of the moon, is highly necessary
 “to the medical practitioner in *India* *.”

VII. *Of securing and extending our knowledge of sol-lunar influence.*

As those discoveries regarding the effects of sol-lunar influence, lead unavoidably to new ideas respecting the nature and cure of fevers, it has become an object of real importance: *first*, to secure the knowledge we have already obtained of this principle; that it may not succumb to any illiberal attempt to suppress or smother it, by representing it as insignificant and useless; or by ascribing to it, the wild and groundless delu-

* Having neglected to apply to the author of this letter for his permission to give his name to the public, and being very unwilling to deprive the doctrine of lunar influence of the support, which it cannot fail to derive from such an evidence; I will now venture to discover, that he is no other than Doctor Hellenus Scott, of *Bombay*. From the information of Doctor Hutton, who resided many years as surgeon at *Penang*; and of Mr James Lumsdaine, surgeon for a number of years at *Fort Marlbro*.; I have now, also, the satisfaction to know, that sol-lunar influence shews its effects in a very conspicuous manner in the prevailing diseases of those Islands †; and that an attention to its laws, is of great importance on conducting their cure.

† Sol-lunar influence has lately been observed by Dr Christie, in *Ceylon*, and by Dr Macgregor, in *Egypt*.—(*New*.)

sions of astrology; *secondly*, to render the road to future observation and further discovery more easy and accessible, by removing the almost unsurmountable obstacles that present themselves, in the intricacy and labour of astronomical investigations; and *thirdly*, to render our knowledge of it so *precise* and well defined, that it may assume the form and attributes of real science, by furnishing precepts for the purpose of applying it to the improvement of useful arts.

1st. To place this theory on a firm and secure foundation, I shall follow the example of the learned Abbé Mann, in his observations on the flux and reflux of the atmosphere †: and shall assume it as a principle requiring no further demonstration, than what it has already received from astronomy, that the influence of that attraction, which regulates the motions of the planetary system, is continually and without ceasing exerting itself, in a proportionable degree, on every particle of this globe; and that it cannot be otherwise.

The existence of sol-lunar influence being demonstrated by astronomy, its action on the human frame is no longer a matter of doubt; and the only question that we have to consider is, not whether that power does actually exist, but whether it manifests itself by the signs of any obvious effect or change in the human constitution.

With respect to this important question, I shall content myself with stating in a very few words, that all the observations I have made myself, together with those that have been communicated by other gentlemen, concur to prove, not merely that sol-lunar influence manifests itself by evident effects upon the human constitution, but that the *attacks, exacerbations, remissions, postponings, and relapses*, of the paroxysms of fevers, which comprehend the whole of the evidence that is necessary

† The Philosophical Magazine, Vol. V. Page 105.

to constitute a complete demonstration, are, in a wonderful manner, coincident in time, and correspondent in degree, with the periodical changes that take place in the power of sol-lunar attraction. To reject, therefore, those accumulated proofs of its actual operation and efficiency, is to violate the principles and rules, by which we infer the existence of a connection or cause, in every question of philosophy, or common occurrence of life.

The proof of regular changes in the atmosphere corresponding with the revolutions of lunar attraction, being now established by the discovery of a regular *diurnal* and *septenary* flux and reflux in the mercury of the barometer, coincident with the diurnal and septenary revolutions of the same power, the theory of sol-lunar influence in fevers receives from this event all the support that can be derived from a fair *analogy*: and it may be inferred with reason, that changes such as these in the element in which we breathe and move, are not likely to take place without corresponding perturbations in the human frame.

The existence of a *diurnal* flux and reflux in the mercury of the barometer, is now sufficiently established by the observations of father BOUDIER*, at *Chandernagore*; of Mr TRAIL, Mr FARQUHAR, and Colonel PEARCE, at *Calcutta*; and those which appear in my Treatise, on the Barometer, inserted in the fourth volume of the *Asiatic Researches*; and on the Coast of *Coromandel*, by the observations of Doctor ROXBURGH†. On the other side of the globe, they have been observed in *South America*‡, and the *West Indies*§; and also at different places in *Europe*||.

* *Traite de Meteorologie*, par Le P. Cotte, page 343.

† Vide the Transactions of the Royal Society, Vol. ———

‡ *Traite de Meteorologie*, par Le P. Cotte, page 399.

§ Doctor Moseley's Treatise on the Diseases of the West Indies, and Le P. Cotte.

|| At *Berlin*, by M. Changeux, vide *Traite de Meteorologie*, par Le P. Cotte, page 618, at *Padua*; by Mr Toaldo and his nephew, vide *Traite de Meteorologie*, par Le P. Cotte, page 616, &c, &c.

The proofs of a *septenary flux* and *reflux*, in the mercury of the barometer, is confirmed by the observations of Mr TOALDO, father COTTE, and others; but still more pointedly by those lately made in *England*, by Mr HOWARD, to be found in a paper read before the *Askesian Society* in *London*, and published in the seventh volume of the *Philosophical Magazine*.

Such is the support and security which the doctrine of sol-lunar influence in fevers derives from evidence *direct* and *analogical*. From the sublime discoveries of LAVOISIER respecting the composition of the atmosphere, it receives protection of another kind. In the present imperfect state of our knowledge regarding the component parts of atmospheric air, and the mode of their combination, who will presume to limit or define its connection with sol-lunar influence? Who will be so hardy and so regardless of his own reputation, as to pronounce without proof, that this influence has *no* power to produce any change whatever in the nature of this compounded fluid, in the smallest degree connected with useful knowledge, or necessary in any respect to be known?

2dly. For the purpose of removing the obstacles that arise from the intricacy and labour of astronomical investigations, in which those who are employed in the study and practice of medicine can have no leisure to engage, it will be sufficient to present a plain and simple idea of this power, with the common changes to which it is liable, abstracted from all the complicated circumstances by which those changes are produced: the consideration of which, though indispensibly necessary for the nicer purposes of astronomy, are by no means required for those of medicine and meteorology.

It was determined by DE LA PLACE*, in 1790, that the force of the moon to excite those perturbations, that manifest them-

* *Astronomie* par Jerome Le Francois La Lande, Tome III, Troisieme Edition, revue et augmentee, additions et corrections, page 737.

selves on the surface of our globe, by the elevation of the tides, is three, and that of the sun one. Assuming this as a foundation, we have only to conceive, that those two quantities of power, sometimes assisting and sometimes counteracting each other according to the varying positions in which they are placed, produce the corresponding changes that are observed in the paroxysms of fevers; remembering, at the same time, that those are occasionally subject to certain perturbations of inferior consequence, from the attractions of the planets. To conceive this, is all that is required.

3dly. To render our knowledge of this principle sufficiently perfect, by giving it all the advantages of *numerical precision*, without which no physical principle can ever acquire the form and efficiency of science, it is necessary that all the various degrees of increase or decrease that sol-lunar influence is liable to undergo at various hours of the day and night, should be accurately ascertained, and *expressed in numbers*.

It is to attain this end that I am now led to propose the scheme of an *astronomical Ephemeris* for the purposes of medicine and meteorology, containing a column for the *horul variations* of sol-lunar power both day and night, ascertained and expressed with all the precision that can be obtained.

The perturbing force of the moon being found by De La Place to be three, and that of the sun one; and four, therefore, being the whole of the perturbing power with which they can act upon this globe, we shall obtain, by dividing this sum into forty parts or degrees, a scale sufficiently extensive and minute for expressing all the different degrees that can possibly occur.

By means of this *Ephemeris*, every phaenomenon that appears being instantly and easily compared with the existing corresponding degree of sol-lunar power, certain general truths will at length be obtained, respecting its agency and interfe-

rence in the different processes of nature, and operations of art. We shall ultimately discover where it counteracts, and where it produces no effects at all; precepts and cautions will thence arise to direct our conduct: and thus assuming the real character and office of science, it will become an instrument of improvement and perfection in the useful occupations of life. In our native country the respectable tradesmen, who are employed in the important national concerns, of supplying our fleets destined for distant voyages and warm climates, with wholesome and durable provisions, are often unaccountably disappointed in the quality of the different articles which they provide. Perhaps they may discover that all the days of the month are not alike favourable for the important processes of brewing, and baking, and of preserving meat. And perhaps abroad, the manufacturers of indigo, sugar, salt-petre, and opium, may find out hereafter that the success of their different operations is not altogether unconnected with certain periods of time.

To those who are proficient in astronomy it will readily occur, that the construction of an *Ephemeris*, such as that which is proposed, is not merely speculative or impracticable. It will occur to them, that there is no hour or division of the column appropriated to the variations of sol-lunar power, for which the precise degree or quantum of its force is not either ascertained by astronomical theorems already demonstrated, or readily deducible from such demonstrations. On those gentlemen, whose studies have qualified them, and whose zeal may incline them, from a sense of its utility, to complete the construction of this instrument, I must for the present rest my hopes. My own imperfect knowledge of astronomy, and the precarious state of my health, render me at this time totally unequal to such an exertion.

CONCLUSION.

In concluding this paper, I hope it will not be deemed disrespectful, if, to prevent future mistakes, I should take this op-

portunity of declaring explicitly my own sentiments respecting the *result* and *success* of these investigations.

“ Having discovered *the laws of febrile paroxysms*, and having marked their course and periods in a manner that was never explained or done before, I conceive that I have been able to unfold a history and theory of fevers entirely new: consistent with itself in every part, and with the other appearances of nature; perfectly conformable to the laws discovered by the immortal Newton; and capable of producing important improvements in medicine and meteorology.”

Should these pretensions prove groundless and visionary, having submitted them to this Society, I shall at least obtain the credit of having sought investigation. If they be fair and just, the harmless vanity of proclaiming them will not obliterate all their merit.



TREATISE VII.

CONTAINING
ARTICLES OF MATTER
DEFENSIVE AND EXPLANATORY
RELATING TO THIS WORK.

THE HISTORY OF THE

REIGN OF

CHARLES THE FIRST

BY

JOHN BURNET

OF

THE

UNIVERSITY OF

OXFORD

TO THE HONOURABLE
WARREN HASTINGS, ESQ.
THIS MEMORANDUM
OF
ATTACHMENT AND RESPECT,
FROM
HIS OLD FRIEND AND OBEDIENT SERVANT
FRANCIS BALFOUR.

Prima dictè mihi—;

Summa dicende Camana.

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ARTICLES
OF
MATTER DEFENSIVE
AND
EXPLANATORY, &c.

ARTICLE I.

To point out to the Editor of the Edinburgh Quarterly Review, of March 1812, several gross mistatements of matter of fact, in his report on my Work, highly injurious to its character.

THESE Mistatements consist chiefly of the following different Articles :

1. A mistatement *of the local extent* of my own personal observations.

2. A mistatement *of the number* of individuals furnishing observations in support of the Sol-lunar Theory ; and a misrepresentation *of their qualifications or competency* to make observations with accuracy.

3. A mistatement *of the means* employed by me to obtain observations from individuals.

The Reviewer, on the ground of these mistatements, having, in a bold and summary manner, rejected the validity of my whole system ; I was induced, on the first appearance of this Review, to enter into a minute and painful refutation of

every article. But as this exposition is of considerable length, and would lead me aside from the more immediate objects of this Treatise; I shall now exhibit only those of the preceding list, as fair specimens of the Reviewer's work : from which the candid reader will be able to judge how far I have a title to expect some explanation on the part of the Editor ; and the adoption of some measure to prevent the farther effects of this injurious and unwarranted report to the public.

1. With respect to the mistatement regarding *the extent of my own personal observations*, it will be found in page 100 of the Review, expressed in the following words : “ We will do Dr Balfour the credit to believe, that his own observations were sufficiently accurate, and not warped by fancy or pre-conceived opinion ; but these *personal observations* prove little more than that at *Calcutta* (a single town) and its neighbourhood, Sol-lunar Influence has an evident effect in the Paroxysms of Febrile Diseases.”

In order to ascertain the truth of this assertion, I must request the reader to refer to the 29th page of my Collection, &c. in which he will find the *extent of my personal observations*, clearly and distinctly described in my own words, more than 30 years ago. “ By *my own personal observations* and experience, I know that the Influence of the Moon at the Full and Change prevails in Fevers from the 13th to the 26th degree of Latitude : that is from the latitude of Madras to the latitude of Lucnow.” This I presume is to say pretty distinctly, that it was not confined to the town of Calcutta, and its neighbourhood, as the Reviewer affirms ; but extended beyond it, a thousand miles on one side, and six hundred on another.

It being evidently my purpose, on the above occasion, merely to specify the farthest limits of *my own personal observation*, and nothing more ; it is equally clear that the specifi-

cation of the different Towns and Military Stations lying between these extreme points was not at all required. Fortunately, however, these have not been altogether omitted: and if the scrupulous accuracy of the Reviewer, before he confined *my personal observations* so very precisely to the town of Calcutta and its neighbourhood, had just led him to turn over a few of the leaves of my book, in order to make sure that they had not gone a little farther, he would have learnt from many different pages, that, besides CALCUTTA, the Military Station of COOCH BEHAR*, distant from Calcutta 300 miles; BANARIS,† 400; CHUNAR,‡ more than 400; and LUCNOW,§ more than 600; are all specified as places of my residence and *personal observation*. And, what will no doubt surprise the reader, and probably the Editor himself, in the very first page of my First Treatise on Sol-lunar Influence, I expressly describe the extent of my own observations to comprehend the whole of the Company's possessions in Bengal; including, also, the dominions of the Vizier.||

2. The mistatement *of the number of individuals* furnishing observations in support of the Sol-lunar System, and *of their qualification and competency* to make observations with accuracy, which will be found in page 100 of the Review, is expressed in these words, “With respect to the Testimonies brought before us we cannot allow *the majority of them much weight* : because the observations were made, *not by Medical men*, or by Gentlemen whose previous education fitted them for making observations in an accurate manner; *they were chiefly made by Officers in the army.*”

These are the words of the Reviewer.—What follows is the Matter of Fact *alias* the Truth: If the friendly reader will

* *Vid.* Collection, &c. p. 30.

‡ *Vid.* Ditto, p. iii.

† *Vid.* Memorial prefixed, p. v.

§ *Vid.* Collection, p. 177 & 178.

|| *Vid.* Collection, p. 29.

take the trouble to refer to the long list of Gentlemen who furnished me with observations contained in page 196 -7 -8 -9, in which the Medical are distinguished from the Military Officers, either by their medical rank printed after the name at full length or by its initial letters, and to the explanatory paragraphs that immediately follow that list, he will discover that the whole number of correspondents or observers of every kind, leaving out Mr Thomas Bainbridge, the *infidel*, was 81: of which number 5 were Civilians; 26, *only*, *Military Officers*; and 50 *Medical men*.—A great *majority* of the whole therefore *were not*, as affirmed by the Reviewer, Military Officers, but *Medical men*; whose previous education and habits peculiarly fitted them for making observations in an accurate manner. It must be remembered likewise, that most of them were educated at the University of Edinburgh; where they did not, probably, receive any preponderating bias in favour of the Sol-lunar System: and what is still more, six of these were Graduates in Medicine from the same University; and two, Members of the College of Physicians.

3. Of all the abuses of the Press, that which endeavours to fix injurious imputations upon conduct, by dark and ambiguous insinuation, is by far the most unfair, dishonourable, and mischievous. In continuation of the memorable judgment in which the Reviewer, so erroneously and confidently asserts, that the observations which I have collected were made by a *majority of Military Officers*, he concludes this famous report in these words, “page 100 and 101,—And Dr Balfour gives “some *curious reasons* why he applied to those Gentlemen *rather than to his Brethren of the Faculty*.”

The whole of this sentence is a shameful and groundless insinuation.—By a reference to the Collection of Treatises, &c. it will be perceived, that the observations collected in the IVth Treatise were obtained by means of a public Circular Letter, addressed by the author to no particular individuals or class of

men, but sent openly and equally to all, without distinction or preference; and it is expressly said in the Preface to that Treatise, page 190, “that it was forwarded to all the Civil and Military Stations in Bengal.”

If there had existed at Calcutta, at the time of publishing my Fourth Treatise, the smallest suspicion, that the information contained in it had been obtained by a partial application to individuals not qualified to give it; or was in any respect not authentic or worthy of credit, is it probable that Lord Teignmouth, then Governor-General, would have directed it to be published and circulated at the expence of Government, “as a work calculated to promote science;” or would have publicly recommended it to the Court of Directors?—And if any imputations of this kind had existed afterwards; and when I had gone to Europe with the declared intention of never returning to India, is it likely, that the following invitation or rather *defiance* published in the Preface, page 190, would not, in the space of 20 years, have brought them forward to light? “In having brought this my first Volume (*i. e.* the IVth Treatise) to an end, I have performed the first part of my task; and in submitting it to the public, where the whole of the Facts, on which my reasoning is founded, have been observed; where the witnesses who testify these Facts are all at hand; and where it is in the power of every individual of the community to observe and examine similar phenomena; to ascertain the weight that is due to every authority; and to state and discuss every question with perfect freedom; I have wished to give it that degree of authenticity and credit, which it cannot fail to obtain with candid and liberal men, from so fair a trial.”

Finally, if any suspicion had been entertained regarding the authenticity of *my information*, or the mode in which it had been obtained, is it to be supposed that the ASIATIC SOCIETY would have allowed a paper, entirely founded upon it,

to obtain a first place in one of their respectable Volumes? viz. Vol. VIII.

Flattering myself, that these corrections will suffice in some respects to counteract the mischievous effects of those mistakes, and to shew that nothing less than the urgent necessity of self-defence has called me forth on this occasion, I shall now proceed to a more agreeable portion of my task;—hoping that I may not soon be obliged to recur to a topic, which is still very far from being exhausted.

ARTICLE II.

To add to the ample Proofs of Sol-lunar Influence, already obtained in Asia and other quarters of the Globe, some decisive evidences of its effects in EUROPE.

THE imperfect account which the Medical Authors of Europe generally afford respecting their observation of the effects of Sol-lunar Influence, have been very unfavourable to the admission of this theory; and appears at present to be the only reason that is offered for rejecting it. It is therefore the object of this small Treatise, to submit to the public a few neglected Documents, which have been extracted from Authors of unexceptionable character and ability, recording *their own personal observations* made in Europe; which appear to me to be perfectly decisive upon this question: Indeed if records such as these are to be rejected, as hitherto, at pleasure, and without any plausible reason assigned, all Medical History is at an end!

The Authors, whose works furnish the following Extracts respecting the effects of Sol-lunar Influence in EUROPE, are Isabrandi Diemerbroeck of Gelderland, and Bernardinus Ra-

mazzini of Modena ; and Dr Mead,* Sir John Floyer,† and Sir Clifton Wintringham,‡ of England : not retailing the Medical Legends of distant countries and ancient times ; but all speaking directly and positively from *their own personal observation*.—Of these Extracts I have given a literal and faithful translation into English.

EXTRACTS FROM DIEMERBROECK.

Diemerbroeck's Treatise on the Plague, which contains his own valuable information, is thus described in the following Title Page : "Isabrandi Diemerbroeck Montforto-Trajectini, ante hac Novio-magi, nunc Ultrajecti Medici, de "Peste Libri Quatuor, Dissertatio, &c. Arenaci 1646."—In the Dedication prefixed to this Treatise, which is addressed to the Prætor and Consuls, and the whole Senate of Utrecht, he describes the nature of his own situation, the opportunities he had of acquiring a knowledge of the disease, and his object in publishing the work, in the following manner.

"As in all well constituted States it is the duty of every
"one to contribute his advice and assistance for the public safety, that by their unanimous concurrence, the present as well
"as impending evils of the State may be averted and repelled,
"I conceive that I should not act improperly, if concerning
"this Plague, of all diseases the most cruel, and more destruct-

* Dr Mead's *own personal observations* mentioned in his works.

† Sir John Floyer's *own personal observations* mentioned in his Treatise on the Asthma.

‡ Sir Clifton Wintringham's *own personal observations*, mentioned in his Commentaries, 19. 66. 74. 75. written after the experience of 40 years Practice in London, and in the Military Hospitals of England.

“ful than any enemy, I too should offer some salutary advice
 “toward the discovery of its hidden nature; together with
 “some more certain method of curing it. For as in warfare,
 “none can so well elude the designs of the enemy or repel
 “his attacks as one who has had experience in the art of war,
 “so none can more effectually resist this cruel disease than
 “one who has intrepidly opposed himself to its fury. This I
 “did a long time ago; not only in the year 1633, when a most
 “violent Pestilential Fever, the forerunner of this Plague, af-
 “flicted most grievously the whole province of Gelderland,
 “and principally the city of Nimeguen, where I was ordinary
 “Physician; and threw upon me so great a load of practice
 “as hardly allowed me to take sustenance: but likewise in
 “1636 and 1637, when the true Plague raged so violently a-
 “mongst the people of Nimeguen, and so great a number of
 “sick was thrown upon my hands as to give me no rest or re-
 “pose.—Having at that time, with great danger, and at the
 “risk of my life, investigated most inquisitively the nature of
 “this most dreadful enemy, I now make public his *Portrait*,
 “delineated in this Book, for the safety of all.”

The whole Treatise is divided into Four Books, each of
 which contains so many Chapters with *Notes* by the Author;
 and under the Fifth Chapter, which follows, will be found the
 principal observations relating to the effects of the Moon.

“CHAP. V.”

“*Of some remarkable things that occurred in the Plague.*”

“Two or three days before and after the (1.) New Moon,
 “and also the Full Moon, this dreadful Pestilence was always
 “aggravated; and it was at that time likewise that most people
 “were taken ill; and those taken ill then almost all died, and
 “that in a very short time: for many, when they scarcely ap-
 “peared to be sick, being overcome by an unaccountable failure

“ of their strength, were extinguished in a few hours; and
 “ many expired on the second and third day.”

“ The Plague in few cases both began and ended without
 “ fever. (2.) In many indeed it began without fever, which,
 “ however, followed soon after. But in the greatest number
 “ it seized with a slight rigour, which fever was immediately
 “ adjoined,—sometimes ardent—sometimes mild—but in most
 “ cases moderate, &c. &c.”

“ Many of the sick died before the 7th day; many expired
 “ suddenly on the 1st day; many on the 3d day; but more
 “ on the 5th and 6th.”

“ Of those who got over the 7th day there was some hope;
 “ yet we have seen some who held out to the 3d, and a few to
 “ the 4th week, and died at last.”

“ Thus the Pestilential Fevers were very deceitful: for the
 “ symptoms being less violent gave great hopes of cure; and
 “ the sick were carried on for a long time apparently without
 “ any symptom of consequence, and were at last cut off, fre-
 “ quently unexpectedly.”

“ *NOTES by the Author.*”

(1.) “ Conformably to what we have observed above, viz.
 “ That in this Plague the disease was greatly aggravated a-
 “ bout the New and Full Moon, so likewise did Joubertus, in
 “ his time, make the same remark.—And hence, also, does
 “ Joannis Morellus, in his book on the Purple Fever, caution
 “ every body to take care of himself at the New and Full
 “ Moon: because, says he, the atmospheric poison is then in
 “ motion. On the other hand Gemma writes, that in the Plague
 “ of the year 1575, the quadrature of the waning Moon was
 “ more powerful. Paræus in his book on the Plague, says

“ that the Waning Moon was often fatal to the sick of the
 “ Plague : which Liddellius likewise affirms in his book on
 “ Fevers, Lib. 3. Cap. 4.—The Waning Moon, he says, uses
 “ to be fatal to the sick of the Plague : for more recover in
 “ the Increase than the Wane, &c.”

The observations of Joubertus and Morellus, quoted above by Diemberbroeck as concurring with his own respecting the Moon, as far as I can judge from the words of the quotation, are expressed positively and distinctly ; and are not contradicted by those of Gemma, Paræus, and Liddellius, as Diemberbroeck seems to think ; but strongly confirmed by them : for the Quadrature of the Waning Moon, which they say was more fatal to the sick, commences at the moment that the Moon passes the Full ; and therefore includes, in its first portion, three and a half of the last days of the Lunar Period : which days in all fevers are well known to be the most severe and fatal.

In short, there can be no doubt, that all these apparently discrepant opinions would be perfectly reconcileable to that of Diemberbroeck ; and to each other, were we possessed of their respective dates.—And to shew that these Facts which he has stated were not adduced to support any particular Theory ; nor from any other motive but that of exhibiting a true and faithful history of the disease, let us hear what he says himself in the sequel of the above *Note*.

“ Concerning this Power of the Moon over Sub-lunary
 “ Bodies there is a vast diversity of opinion, which it would
 “ be too long to discuss.—Amongst those, however, *that* seems
 “ to be nearest the truth, which supposes the Moon to con-
 “ tribute nothing towards the rising of the Tides, humours,
 “ juices, &c. &c. but that these, in accord with nature, are
 “ moved at the same time, along with the Moon ; and that the
 “ Moon is only the SIGN of that motion, and not the EFFI-
 “ CIENT CAUSE.”

This opinion he repeats and explains much more fully afterwards, in Cap. III. p. 24. So that we may safely acquit him of recording the preceding Facts from any partiality to Sol-lunar Influence. In Cap. XV. on the Prognosis, Art. II. p. 76. he says further :—" Whosoever were seized with the " Plague about the New Moon, were in the greatest danger " of dying."

EXTRACTS

FROM RAMAZZINI.

The work in which Ramazzini introduces his observations respecting the effects of the Moon in Fevers, and to which I now refer, is the Author's History of the Epidemic Constitutions that prevailed at Modena, and the neighbouring country, in 1690 -1 -2 -3 -4; but particularly his Dissertation on the Epidemic Constitution of the three last of these years,...which is entitled, " De Constitutionibus trium sequentium annorum " 1692, 1693, 1694, in mutinense civitate, et illius Ditione, " Dissertatio." Which Dissertation will be found in the 1st volume of his *Opera omnia Medica et Physiologica*.

After having sat twenty years in the chair of First Professor of Theory in the University of Modena, he became afterwards Primary Professor of Practice in the University of Padua. From his election to these elevated situations, we cannot but suppose, that he was a man of distinguished ability and respected character; and by no means likely to form a connected series of falsehoods for the purpose of supporting a Theory.

The DISSERTATION to which I refer begins in these words :

" SECT. I.—In resuming the work which I had begun, but " from which, owing to various causes, and particularly indif-

“ferent health, I was obliged to desist, I shall describe in a
 “compendious manner the Diseases that prevailed Epidemically in this quarter during these three years ; together with
 “some uncommon circumstances that appeared to be worthy
 “of remark : trusting that, if the subject should not be treated in the manner it deserves, I shall be generously forgiven :
 “provided it be done correctly, which is the principal requisite in historical narrative ; and that I engage to perform religiously, &c. &c.”

“SECT. XIII.—But the Epidemical affections which, during these three years, so very unlike each other in their constitutions, afflicted the city, villages, and neighbouring towns
 “with numerous funerals was the Purple or Petechial Fever ; which sometimes, after committing great havoc, seemed
 “inclined to sound a retreat, but soon rushed on again with
 “augmented force, and spread itself every where.”

“This Fever first made its appearance about the Vernal Equinox of 1692, and advancing at the beginning by slight attacks, soon after put on its Petechial garb ; and raged with
 “severity during the whole of the spring and part of the summer, until the beginning of the dog-days. So that, when
 “it became sultry, this Fever laid aside the Purple, and did not
 “again resume it until the summer heat began to remit, that
 “is about the rising of Areturus (in July) ; which mode it
 “likewise observed strictly the following years,—changing its
 “garb indeed, but not in any respect its nature and course :
 “for the Fevers that appeared in the greatest heat of summer
 “without Petechiæ, were not the less malignant in their disposition ; which the Vibices appearing after death sufficiently evinced.”

“SECT. XIV.—But it was remarkable that this Fever
 “raged particularly after the *Full Moon*, and much more so
 “when the Moon was at the *Change*, and became milder with

“ her new light. Nor was this the constant observation of
 “ myself only, but of the other Professors : and it assisted us
 “ not a little in forming the prognosis and directing the cure.
 “ But whether the Moon has any peculiar Influence or Power
 “ over Epidemic diseases, in consequence of which they are
 “ obedient to her Phases and various Changes, is a question
 “ that merits attention.”

“ SÉCT. XV.—But certainly it was a most surprising thing
 “ that on the 21st of June, 1693, a Lunar Eclipse * having
 “ happened in the night, the greatest part of the sick died;
 “ and almost at the very time when the Moon was darkened :
 “ and some were carried off without any warning, &c.”

In the sequel of the above Section, the author details another remarkable instance of the effects of an Eclipse on a sick Lady at Paris, observed by Ballonius : and having in the beginning of the next Section, described the striking effects of another on the Mercury of the Barometer observed by himself, he proceeds to infer :—

“ SÉCT. XVI.—That since, in the opinion of Hippocrates,
 “ we ought to be cautious how we do any thing of consequence
 “ about the Equinoxes and Solstices, it would not be amiss if
 “ the physicians and surgeons of hospitals should turn not only
 “ their corporeal eye but their mind’s eye towards Eclipses.”

And then, as if he were afraid that this declaration might be converted into a charge of astrology, he adds, “ I hope,
 “ however, that nobody will believe it to be my opinion that
 “ indications for curing the sick ought to be taken from the

* It must be remembered, that Eclipses always happen at the Full of the Moon ; at which juncture Sol-lunar Influence, agreeably to the Theory, has uncommon power. The Eclipse, or interception of Lunar light, is merely a collateral circumstance, and probably has little or no effect.

“ various astrological aspects or configurations of the heavenly bodies : for, when the patient is in danger, it is wiser, as Mirandus says, to consult the urine than the stars ; and to give appropriate medicine, although the firmament should protest against it, &c. &c.”

These Extracts from Diemerbroeck and Ramazzini must be allowed to agree in a surprising manner with each other ; and to mark out, with wonderful precision, the limits that I have been led to assign to the Lunar Periods and Inter-lunar Intervals ; from similar phenomena observed in similar diseases in India and other places. And I am likewise induced to believe, that these testimonies, when fairly and candidly contemplated as the report of creditable men having no interest or wish to impose, cannot fail to impress an unbiased mind with the belief of a power or cause connected with the Phases of the Moon, existing and acting in EUROPE as well as in ASIA.

But as men of ordinary understanding contemplating these Facts loosely, and deducing opinions from them by the usual process of common sense in common affairs, may form conclusions differing from those of the logician and philosopher ; let us endeavour to ascertain by a more profound and rigid application of the Reasoning Faculty, and according to the Theory of Probabilities, as explained by Demoivre, Laplace, and other distinguished men, what is the precise conclusion that we are naturally impelled and entitled to draw from these Extracts.

Ramazzini, who had to do with Fevers nearly resembling those of Bengal and other parts of India, will be readily allowed, considering the extent of his field, to have observed, in the course of the three years during which this Epidemic raged at Modena, the successive septenary Lunar changes of

at least 8 different cases of Fever ; each commencing with the Lunar period and continuing 21 days, which is a common duration in fevers of this kind. He would therefore observe 4 different striking *coincidences* between the septenary changes of each Fever, and the Phases of the Moon in the following manner :

In Case 1st. /

The *Attack* at the beginning of the *Lunar Period*, - - 1

The *Remission* at the beginning of the *Inter-lunar Interval*, 1

The *Attack* at the beginning of the *Lunar Period*, - - 1

The *Remission* at the beginning of the *Inter-lunar Interval*, 1

								4
In Case 2d. ditto,	-	-	-	-	-	-	-	4
In Case 3d. ditto,	-	-	-	-	-	-	-	4
In Case 4th. ditto,	-	-	-	-	-	-	-	4
In Case 5th. ditto,	-	-	-	-	-	-	-	4
In Case 6th. ditto,	-	-	-	-	-	-	-	4
In Case 7th. ditto,	-	-	-	-	-	-	-	4
In Case 8th. ditto,	-	-	-	-	-	-	-	4
Total, - - -								32

Ramazzini, therefore, had occasion to observe no less than 32 remarkable *coincidences* of Fever with certain positions of the Moon, occurring in a regular and constant series of succession, agreeably to the statement above.

This point being established, let us suppose that, on its being observed that the *Attacks* constantly and invariably corresponded with the *Lunar Periods*, and the *Remissions* with the *Inter-lunar Intervals*, a question should arise whether that regular coincidence ought to be ascribed to *chance* or *design*.—It would be found by an appeal to the infallible rules of the Doctrine of Chances, that there are the odds of 263313083 millions of millions of millions of millions to 1 that these coin-

evidences were determined to the order in which they were found by design,* or by the power of some established general law of nature :—Not to a law of nature with which the phenomena have no connection, but to one with which they are inseparably connected, and to which they constantly point.—*i. e.* to Sol-lunar Influence.

Such, then, is the only true and accurate method of estimating the value of evidence relating to the possibility of contingent events ; and such is the conclusion we are obliged to draw from the *personal observations* of Ramazzini alone, supposing them to be limited to 8 cases of Fever, and no more.

It must, however, be obvious, that the intrinsic value of the evidence to be drawn from a similar calculation of the personal observations of his cotemporaries, the Physicians and Professors of the College of Modena, to whom Ramazzini is addressing himself, and who, he says, made the same remarks, and regulated their prognosis and cure by it for three years together, cannot be estimated at a lower rate : nor is it possible to put a lower value on the Extracts which have been taken from Diemerbroeck's work.

Besides these we might proceed to analyse the personal observations of Galen, in former days, and in later times those of Dr Mead, Sir John Floyer, Sir Clifton Wintringham, Dr Rutter,† and many others affording evidence of Sol-lunar Influence in *Europe* equally unexceptionable. But for the purpose of this investigation, I have chosen to confine myself more particularly to the Evidences of Ramazzini and his Colleagues at Modena and those of Diemerbroeck ; because they are accompanied with a train of collateral circumstances that

* *Vide* De Moivre on the Doctrine of Chances,—Preface, page vi. &c.—and Laplace's " *Theorie analytique des Probabilites*," Livre II. Chap. Premier.

† *Vide* Collection, page 299.

places them beyond the reach of cavil. And being better defined in time and place, they are more easily subjected to the process of this calculation: which I have been anxious to illustrate by this example; seeing that it will serve, not only to establish a decisive demonstration of the existence of Sol-lunar Influence in EUROPE as well as in ASIA, but will supply the fair and candid inquirer with *the formula of a criterion* to which he may always appeal, from the outcry of ignorance, and the arbitrary voice of bare opinion and authority.—And being a mode of decision prescribed and sanctioned by the distinguished author of the valuable work to which I have referred, and other eminent Philosophers, it is less liable to be rejected on a frivolous pretence.

But this is not the only instance in which the cause of Sol-lunar Influence is indebted to this distinguished Philosopher. He has already expressly acknowledged* the probable existence of this power; and the confirmation he has given of the reality of the DIURNAL VARIATION of the Barometer, and that too in Latitudes far beyond those to which it was not before allowed to extend, is of the utmost importance to this Theory. The reality of a constant diurnal and nocturnal fluctuation in the Mercury of the Barometer, contemporary with that which takes place in the state of Fevers, being established on such authority; no Physician who has any regard for his own character, will now propose to carry on his investigations and researches in the different branches of medicine and philosophy, without some attempt to account for phenomena so inseparably connected with his subject, and so very important in their nature.

As the substance of Laplace's observations, relating to the DIURNAL VARIATION of the Barometer, has been extracted in a condensed form by the Edinburgh Review for September

* Vide Laplace's "Theorie analytique des Probabilites," page 358.

1814, NO. XLVI, pp. 333 -4.—I shall take the liberty of inserting it in this place.

“ Of this application of the Doctrine of Probabilities, a number of Examples are then given. The first relates to the Diurnal Variation of the Barometer, as found from the observations of that Instrument made at the Equator, where it is least subject to the action of irregular causes. From these there appears to be a small diurnal oscillation, of which one *maximum* takes place about 9 in the morning, and a *minimum* about 4 in the afternoon; a second *maximum* at 11 at night, and a second *minimum* about 4 in the morning. The oscillations of the day are greater than those at night, and amount to $\frac{1}{10}$ th of an inch. The inconstancy of the weather does not allow this variation to be immediately observable without the tropics; or within the range of the variable winds. Nevertheless, by applying the calculus of probabilities to a great number of accurate Barometrical Observations made by Raymond, during several successive years, Le Compte Laplace has found such indications of the same oscillation, as to have no doubt of its existence, though concealed under the irregular action of many accidental causes, &c. &c.”

These observations of M. Le Compte Laplace are confirmed by a valuable ABSTRACT of observations on the DIURNAL VARIATION of the Barometer between the Tropics, by J. Horsbrugh, Esq. published in the Philosophical Transactions of London, for 1805, part I.—Which establishes the existence of a similar but more distinct variation in the oceans of the northern and southern hemispheres; extending to the 27th and 28th degree of latitude, and from the longitude of Britain to the China Seas.

For the purpose of farther establishing Sol-lunar Influence as an universal law in nature, nothing now appears to be want-

ing, excepting a few direct observations of its effects in the extensive oceans that separate the different terrestrial quarters of the globe; and with this view I shall now insert the following valuable document from an experienced and most respectable Surgeon, who fell in the late Marhatta war, doing his duty in the field of battle. It entirely coincides with what I have experienced in my own person returning sick in three different voyages from India to Europe; and in the persons of many of my fellow passengers.

“ SIR,

“ Since arriving in Bengal, I have had various opportunities of perusing your investigation concerning Solar Influence in Fevers; and have been much pleased to find a fact, so extraordinary in itself, and of so much consequence in practice, established in so great a degree, as to require little if any more authority to render it equally well founded with any axiom in medicine.”

“ I cannot however resist the inclination I have to inform you of a Fact which, I humbly apprehend, at a more early period of your inquiry, would have had some weight; and even now may tend to shew that this Influence is as extensive as it is certain.”

“ In my first voyage to sea, as Surgeon of an Indiaman, I soon began to perceive, that the Intermittent Fevers were more irregular than I had observed them before. This became more remarkable as we approached the warmer latitudes; and in the straits of Banca, on our way to China, so many cases occurred as to convince me that there was some uncommon cause for their recurrence in men, who, having been free from the attack for several days, had returned to duty, and were apparently in high health; that I was led to pay more attention to the subject; and without the most distant idea of the Influence of the Heavenly Bodies, (for I

“ must candidly confess that I never heard of such a thing).
 “ The stated periods of Full and Change of the Moon so constantly corresponded with the renewed paroxysms, that I
 “ had not a doubt in my own mind, that this, however unaccountable to me, was the true cause that produced the effects which I have taken the liberty to mention. And so
 “ much was I convinced of this fact, that on returning to England I could not refrain from inserting an account of it in
 “ my Medical Journal ; which was inspected by a Surgeon at the end of the voyage : from whom I had the satisfaction
 “ of learning, that my observations were not ill founded.”

“ I will only take the liberty of adding that, in my subsequent voyages, which were three, this Influence appeared
 “ to me as strong and clear as any Fact could well be, you
 “ may rest assured, that is most faithfully recorded.*

“ I am, SIR, &c. &c.

(Signed) “ THOMAS LYON.”

“ *Fort-William, Royal Barracks, 10th Aug. 1795.*”

“ F. BALFOUR, ESQ. &c. &c.”

It is now highly gratifying to me to conclude these documents with the testimony of Mr Johnson, the respectable author of the Treatise on *Tropical diseases* ; to which I have alluded in the Preface to this volume : and who expresses himself, with respect to Sol-lunar Influence, in the following decided and comprehensive manner.—*Vid.* ESSAY ON TROPICAL CLIMATES, page 88, and other places.

* In confirmation of the evidence contained in this letter, of the effects of Sol-lunar Influence observed at sea, many other observations will be found in the testimonies of the IVth Treatise of this Collection ; and likewise in the Journals deposited by other Surgeons of Indiamen at the India-house.

“ The last existing cause which I shall mention is the Influence of the Sun and Moon.—However sceptical Professional men in Europe may be in regard to Planetary Influence in Fevers, &c. it is too plainly perceptible between the Tropics to admit of a doubt. I have not only observed it in others, but felt it in my own person in India, when labouring under the effects of obstructed liver.” And again; he says in page 89, “ That this Influence pre-disposes to, or exacerbrates, the paroxysms of Fevers in India, and other Tropical climates, is incontestibly proved by daily observation: &c. &c.”

ARTICLE III.

To attempt an explanation of the peculiar nature of the 4th day in Fevers; which has so much puzzled Physicians, both ancient and modern.

HAVING removed the obstacles, which have been the subject of the two preceding Articles of this Treatise, and which have been opposed with so much art and so little candour to the doctrine of SOL-LUNAR INFLUENCE; I now conceive myself perfectly at liberty to assume it in all future discussions as an UNIVERSAL PRINCIPLE in the operations of nature; not less firmly established than those of Gravitation, Light, Electricity, Magnetism, or any other.—And there is no Physician now, that does not wish to shut his eyes and also his conscience, who will not perceive and acknowledge, that the lengths or durations of Fevers; and of course the times of their crises and terminations, are no other than those that are measured and marked out by the various positions of the Sun and Moon; and which were defined and arranged in the Nosological Table of page 91; and delineated in the DIA-

GRAM that was given to the public in the Second Edition of this work, p. 96.

But whilst it is observed, that these Fevers differ essentially from each other according to the modifications which they receive from the fluctuating impressions of SOL-LUNAR POWER, it must not be forgotten that they are likewise subject to another modification ; depending on the different CONSTITUTIONAL PRE-DISPOSITION OF HUMAN FRAME peculiar to each individual.

To develop, therefore, distinguish, and comprehend justly, in every individual case, the particular combination of these TWO DISTINCT ELEMENTARY PRINCIPLES, and to prescribe accordingly, constitutes the whole business of the Physician.

But it is far from my purpose at present to engage in the whole detail of this comprehensive field. The immediate object of this article is to explain the nature of certain equivocal circumstances, peculiar to the 4th day of fevers commencing with the beginning of the Lunar Period ; which having the appearance of a natural crisis, may seem, if not duly understood, to contradict the general character which fevers beginning and having their course to run in the Lunar Periods ought to maintain, agreeably to the power ascribed to those periods by the Sol-lunar System ; and thus to furnish evidence of its want of consistency and truth. This necessary explanation, therefore, I shall now attempt.

Having had occasion, during my long residence in India, to meet with these equivocal occurrences, and to give them attention in a very great number of cases of the common Remittent Fever of Bengal, I can speak of them with confidence. They generally take place in cases of the fever beginning early in the Lunar Periods ; that is to say in the diurnal or nocturnal meridional period of the 1st day ; and we may, there-

fore, infer strongly pre-disposed to febrile paroxysm. The daily increasing power of Sol-lunar Influence acting in this pre-disposition, produces in the course of each day, one or two paroxysms with augmented force.—So that in the nocturno-meridional period of the 3d or 4th day the patient has to undergo a 6th, 7th, or 8th paroxysm, more violent than any before it: and it is often a very severe and doubtful conflict.—In the course of the morning, this nocturnal struggle is followed by a remission more distinct than usual, accompanied perhaps with discharges of bile from the stomach, evacuations downwards, perspiration, and some change in the urine. These appearances afford, at first, some promise of a final crisis; but nothing is completed.—The febrile paroxysm returns in the course of the same or next day: sometimes with less violence, but oftener with more; and continues its exacerbations (if the patient be not carried off before,) to the expiration of the LUNAR PERIOD in which it commenced: When the 7th or 8th day produces a decidedly critical and favourable change.

It is therefore evident, that there is no just ground to infer, from these occurrences on the 4th day of fevers commencing *with the beginning of the LUNAR PERIOD*, any thing like a real crisis, inconsistent with the established laws of Sol-lunar Influence at this stage of the disease. And it is obvious that Hippocrates and Galen committed a mistake in considering the 4th day in all Fevers indiscriminately a natural critical day; and in thus classing it with the 7th, 14th, and 20th; which by the discovery of the Sol-lunar System are now marked out and distinguished as such by the hand of nature herself.—We must, however, do these renowned ancients the justice to observe, that it was not without hesitation or doubt, or without having noticed the anomalous appearances we have described: for it was from observing these that they were led to give it the appellation of INDEX to the 7th day.

But there are other circumstances besides that might na-

turally lead Hippocrates into this mistake : which are, that the 4th day, in some *particular cases*, is in reality a natural critical day—receiving its form, like the rest, from the impression of Sol-lunar Influence. The particular cases to which I allude are those in which the pre-disposition to febrile paroxysm not being so strong or mature as in those that attack on the 1st day, does not shew itself until the 4th or 5th day of the LUNAR PERIOD, when the fever, owing to its weaker pre-disposition, proceeds with more moderate symptoms, and terminates, or rather subsides, on the 4th day from its commencement ; that is on the decline of the Lunar force, after the expiration of the period.

Under the head of *particular cases* come also to be included, fevers that, having apparently terminated for weeks together, appear again about the middle of the LUNAR PERIOD, and again subsiding with its declining Influence exhibit similar cases of crisis on the 4th day.

Having thus explained, “ that the occurrences of the 4th day in fevers, whether commencing in the *beginning* or the *middle* of the LUNAR PERIOD, are perfectly consistent with our Theory of Sol-lunar Influence,” I have accomplished the object of this discussion.

But in the process of establishing this conclusion, we have acquired the knowledge of another proposition, which must not pass unnoticed, and which is this ; “ That in fevers commencing with the Lunar Period, the human frame is not in general able to endure the effect of 6 or 7 reiterated paroxysms, without manifesting, by the laws of its constitution, *appearances of a critical struggle*, such as those that I have described.”

As it is reasonable to suppose, that this proposition respecting the common or general constitution of the human

race must hold good, to a certain degree, in all climates; it naturally leads us to inquire how far this *constitutional tendency* concurs with Sol-lunar Influence, in modifying the *Forms* of the Febrile Diseases of EUROPE.

In this Inquiry we are led to notice the following remarkable circumstances respecting the *TRIBE* of *EXANTHEMATA*; which constitute a large proportion of the Febrile class in Europe.

1st. That in this Tribe the Eruptions, how-much-soever apparently different in their nature from each other, break out with wonderful uniformity in all, nearly about the 4th day: and after a considerable struggle, similar to that of the 4th day of the Fevers in India commencing their attack at the beginning of the Lunar Period; and under the *modification* of a strong Sol-lunar Influence.

2d. That the Febrile Paroxysms of this Tribe are apt to continue returning after the 4th day, and after the Eruption has been completed, until the 7th and 8th day, when the symptoms of a decided crisis have made their appearance: Thus corresponding with the Remitting Fevers of India beginning with the Lunar Period, and going through it, without stopping, to their termination about the 7th or 8th day; agreeably to the established Laws of Sol-lunar Power.

3d. That the Exacerbations in the Small Pox, about the 11th day, correspond very nearly with that frequently observed in the Remitting Fever of India, about the same stage of the disease, and referable only to the Laws of the Sol-lunar System.

4th. That, in certain cases of the Small Pox in Europe, it is frequently the tendency of the Fever to run on after the exacerbation of the 11th day, and to terminate on the 14th or

20th, or some intermediate critical day; as in the common Remitting Fevers of India, directed in their course by Sol-lunar action.

5th. That, although these observations are more particularly obvious in the history of the SMALL POX in Europe, as delivered by the best Medical Authors, similar *resemblances* are likewise to be found in the history of the MEASLES and SCARLATINA, &c. And although my own personal observation of these diseases both in Asia and Europe is very limited; yet have I seen enough, and even in the Cow Pox, to convince me that they are all more or less subject to the dominion of Sol-lunar Power.

This opinion, with respect to the SMALL POX, I communicated to the public more than 30 years ago, in my First Treatise * on Lunar Influence: and along with it my determination to avoid the dangerous concurrence of the Eruptive Fever with the Lunar Period, by inoculating on the 2d or 3d day of this Period; so that the Eruptive Fever might take place in the Inter-lunar Interval.—This resolution I afterwards carried into effect with perfect success, by inoculating, at Banaris and Chunar, four different children.—But it must not be concealed, that the truth of the doctrine manifested itself by proofs, that were rather more decisive than I desired. In three of them the Eruptive Fever was so inconsiderable and the Eruption so tardy, and attended with so much depression, that I was truly alarmed.—All these symptoms however soon went off, and the disease terminated in all of them in the most favourable manner: yet I certainly should not have repeated the experiment without some other modification. But the happy discovery of Dr Jenner soon extricated me from this dilemma: and although the story be now divested of all its interest, yet the Fact should not be lost, and I have not hesitated to give it place.

* *Vide* Collection, p. 43 and 177.

But it is now time to close these suggestions respecting the possible interference of Sol-lunar Power in the Febrile Diseases of Europe. Destitute of comprehensive CLINICAL RECORDS, sufficiently minute and accurate to ascertain the coincidence and non-coincidence of the phenomena with their *precise dates*, we shall at present suspend our attempts to proceed any farther : confiding, in the meantime, that the dispensation of truths so important to science and to mankind will not long be partial.

Orte BERKLEYO ! aio te potentem

Pandere cælum : nec dubitandum

*Tua quin affulsit, Lux Sol-lunaris.**

* Ex Fan. Regul. Oracul. Frag.

APPENDIX.

THE Publication of Dr Jackson's Treatise on the Fevers of Jamaica in 1791, on a subject so nearly allied to that of my two first Treatises, and respecting which Dr Jackson had already published the observations to be found at page 278 of this volume, could not fail to excite my curiosity. It was accordingly perused with avidity, and drew from me the following postscript, originally annexed to my III^d Treatise. That postscript being omitted in its original place, with the intention of examining it more minutely, is now inserted in this Appendix ; which I have set apart for the purpose of agitating whatever matters may become, hereafter, subjects of controversy.

Considering, however, that the different proofs and discussions accumulated in this present volume will sufficiently exclude the *pretensions* of any other competition, to the discovery of this system, or any of its essential parts ; and as Dr Jackson after an interval of 20 years, though formally invited, has not thought it advisable to attempt a solution of the inconsistencies pointed out in this Postscript. I shall now defer that species of rigid analysis which these seem to require ; and shall only request of the discerning and inquisitive reader to contemplate with attention, the irresistible arts by which, on a distant island, he won the first favours of the innocent unguarded *Crisis* * ; and the exquisite address by which he at last overpowered the hitherto inflexible virtue of the wizzard

* Dr Jackson's Treatise on the Fevers of Jamaica, &c. Chap. III. from beginning to end.

Dame MEDICATRIX NATURAE † ; and ultimately to compare these classical *Episodes* with the simple history of the same events contained in this collection §.

A POSTSCRIPT containing remarks on Dr Jackson's *Treatise on the Fevers of Jamaica*, and originally annexed to my *IIId. Treatise*.

HAVING just now discovered* that Dr Jackson, in his *Treatise on the Fevers of Jamaica*, published in April or May 1791, has done me the honour to mention me as the author of a *Treatise on Lunar Influence*, published at Calcutta in 1784, and has entered into a criticism of the doctrine delivered in that treatise, as if it were the latest and only opinion I had ever published on this subject, I cannot resist this opportunity of expressing my regret, that the learned Doctor had heard nothing of my *Treatise on Putrid Intestinal Remitting Fevers*, published in February or March 1790, more than twelve months before his book.

In this *Treatise on Putrid Intestinal Remitting Fevers*, my first imperfect notions respecting lunar influence are altered and extended, after the additional experience of five years, with opportunities of observing, that do not fall to the lot of every practitioner. And, when I reflect that it was advertised and sold in London so early as June or July 1790, that it was taken notice of in the *English Review* for October 1790, and that an account of it appears in Dr Duncan's *Medical Commentaries*, published at Edinburgh about the end of the same year, I cannot help considering myself as very unfortunate, that it did not fall into the Doctor's hand in sufficient time to prevent his censure of my first publication.

† Ditto Ditto. Chap. IX. Sect. I.

§ *Treatise Ist, IId, and IIIId*, all published before Dr Jackson's Book.

* Vide the Notes at the end of his *Treatise*, page, 18.

Having, in my preface to my last Treatise, announced the design of considering the different systems that have been written on fevers, in a second volume, it is not my intention now to anticipate that work, by entering into a review of Dr Jackson's Treatise; it, however, seems expedient, at this time, to make a few remarks upon it.

Uniting the idea of a septenary revolution in fevers, with a tendency in their paroxysms to assume particular types, or, in other words, to attack and remit at stated intervals†, Dr Jackson forms a theory, which, he conceives, enables him to explain satisfactorily all the circumstances that relate to critical days: And these septenary revolutions which Dr Jackson has adopted, are entirely independent of the revolutions of the moon.

If Dr Jackson can establish the existence of two sorts of septenary revolutions, the one connected, the other totally unconnected with the lunar periods and intervals, we must acknowledge that he has made a new and important discovery in medicine, the merit of which is due to him alone.

But, on the other hand, if Dr Jackson's septenary revolutions were connected and coincident with the lunar periods and interlunar intervals, it will then follow that the theory, by which he proposes to account for the critical days of fevers, does not differ in any material respect from the theory suggested in our first Treatise in 1784, and afterwards fully explained in our last Treatise in 1790.

A reference to the dates of these septenary revolutions, in all the different cases in which they were observed, would have settled this important question at once. But, unfortunately, the Doctor's circumstantial accuracy has failed him on this occasion. Like the father of physic, whom he repre-

† Vide Dr Jackson's Treatise, page 40, 65, 67, 68, &c.

hends for a similar neglect‡, he has forgotten to specify the dates of the septenary revolutions which he observed; and the reader is left in the dark, deprived of the only criterion by which he could have formed an opinion for himself with certainty.

Whatever Dr Jackson's sentiments may be, at present, respecting this important point, we will venture to say, that there was a time when he certainly considered almost all* the relapses and returns of fevers at distant periods observed by him (in other words, their septenary revolutions,) as connected with the lunar periods, and dependent on the revolutions of the moon; *and when he did not ascribe them to any other cause.*

In proof of these observations, I beg leave to refer the reader to a paper written by Dr Jackson, and published in the London Medical Journal for 1787, Vol. 8. Part First, under the following title: "*Some Observations on the Connection of the New and Full Moon, with the Invasion and Relapse of Fevers, by Robert Jackson, M. D.*" The whole of this paper is worthy of attention, but especially the strong and pointed passages contained in pages 29, 31, 32, and 33§.

Since the publication of this paper, Dr Jackson's sentiments on this subject seem to have undergone a remarkable change. Those strong and pointed expressions respecting the influence of the moon, which I have recommended to the reader's attention, do not appear in his late work; the connection of the relapses and returns of fevers with the periods of the moon, are mentioned as if it were only by accident, and with reserve; and we now discover, *for the first time, the term and doctrine of*

‡ Vide Dr Jackson's Treatise, page 51.

* Vide Dr Jackson's Paper on the Influence of the Moon in the London Medical Journal for 1787, page 32.

§ The whole of the Paper here referred to, will be found at page 278 of this Volume.

the septenary revolutions independent of the revolutions of the moon, with an attempt to employ them in explaining the crises of fevers.

Why Dr Jackson might incline to introduce a term in treating of this subject, different from that which he used in his first paper, is a circumstance for which it might perhaps be easy to assign some reason: But, upon what grounds he has ventured to advance an idea of the thing itself, so opposite and incompatible with what he expressed before, is beyond our conjecture. He has not been a second time in Jamaica or America; he has collected no new observations in that quarter; and, indeed, both publications are avowedly composed from the same materials. A review of the notes and memorandums which he had saved, and the recollection of those which he had lost, first led him to conclude, that the relapses and returns of fevers at distant periods which he had observed, (in other words, their septenary revolutions), were almost always connected with the lunar periods. But, how a second review and recollection of the very same notes, after an interval of four years, should incline him to infer that the revolutions which he observed were not almost always connected with the lunar periods, but with periods totally distinct and independent, *and never before mentioned, or even insinuated in his first publication*, is a circumstance which we do not comprehend, and which we leave for the Doctor to explain.

The intermediate interlunar crises which we have defined in our 11d Treatise on Fevers, page 34,* are very often succeeded by a paroxysm commencing with a cold fit† and other ap-

* Vide page 100 of this Volume.

† In treating these fevers, it is of importance to know that, about the time of the intermediate interlunar crises, uncommon secretions of bile are apt to take place, and to be accumulated in the intestines; and it is to this cause that I refer the cold fits which frequently shew themselves at these stages of the disease. When the bile is instantly evacuated, the fever often, in such

pearances, totally different from that which they assumed in the preceding lunar period ; and which we have shown in our II^d Treatise, page 34‡, is a character peculiar to the paroxysms of the interlunar intervals. An apparent recommencement of the fever, with a change of its character, similar to that which we have observed to follow the *intermediate interlunar crises*, is described by Hippocrates in almost every case he has given us of any length ; and a similar apparent recommencement of the fever, with a change of its character, has also been observed by Dr Jackson. But the particular period or interval of the moon, at which these events took place, are neither specified by Hippocrates, nor by Dr Jackson ; and, from his neglecting to attend to this circumstance, we presume that the Doctor has permitted himself to be led astray by his new idea of *septenary revolutions independent of the periods and intervals of the moon*. Dr Jackson, to support his new doctrine, may perhaps be able to discover hereafter, by a reference to his notes, that these events were totally unconnected with the periods and intervals at which I have observed them. But, with what propriety will he then assume the histories of Hippocrates to support his doctrine, when their dates cannot possibly be ascertained in such a manner as to establish their exact coincidence, in point of lunar time, with his observations ?

Whatever difference may exist between Dr Jackson and me respecting the cause of septenary revolutions, it gives me great satisfaction to find, that both of us concur in ascertaining a fact which has been so little attended to, *or rather totally overlooked by modern physicians*. And I must again lament,

cases, terminates finally, or at least proceeds with moderate symptoms. But, if the evacuation of the bile be neglected, or deferred too long, a bilious fever is superadded to that which existed before ; and this was evidently the case in many of the histories delivered by Hippocrates ; where the bile was not carried off by a natural looseness.

‡ Page 83 of this Volume.

that he had not met with my Treatise on Putrid Intestinal Remitting Fevers, in which the laws of the septenary revolutions of these disorders are not only minutely described in language, but delineated and illustrated by three different tables constructed for that purpose.

The laws which I have explained, are inferred by a regular induction from phaenomena which are obvious, uniform, and universal. From the uniformity, universality, and constancy of nature in all ages, we infer, that they were the same in the days of Hippocrates that they are now; and, consequently, that the histories of different cases of fevers to be found in his works, which in form are similar to the fevers of the present day, and therefore reconcileable and referable to the influence of the same laws, are nothing more than so many examples of their power and energy 2000 years ago; and cannot, consistently with the rules of philosophy, be referred to any other cause.

With regard to the septenary revolutions observed by Dr Jackson, presuming that the judgment which he formed upon his own observations in 1786, (when his recollection was fresh and entire, and when he decided that they were almost always connected with the revolutions of the moon,) is more to be depended on than that which he formed in 1791, we cannot hesitate to draw the same conclusion that he did himself at that time.





